SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

FRIDAY, MARCH 1, 1907

CONTENTS

MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

THE ENERGIES OF MEN[1]

WE habitually hear much nowadays of the difference between structural and functional psychology. I am not sure that I understand the difference, but it probably has something to do with what I have privately been accustomed to distinguish as the analytical and the clinical points of view in psychological observation. Professor Sanford, in a recently published 'Sketch of a Beginner's Course in Psychology,' recommended 'the physician's attitude' in that subject as the thing the teacher should first of all try to impart to the pupil. I fancy that few of you can have read Professor Pierre Janet's masterly works in mental pathology without being struck by the little use he makes of the machinery usually relied on by psychologists, and by his own reliance on conceptions which in the laboratories and in scientific publications we never hear of at all.

Discriminations and associations, the rise and fall of thresholds, impulses and in-

[1] Delivered as the presidential address before the American Philosophical Association at Columbia University, December 28, 1906.

hibitions, fatigue,—these are the terms into which our inner life is analyzed by psychologists who are not doctors, and in which, by hook or crook, its aberrations from normality have to be expressed. They can indeed be described, after the fact, in such terms, but always lamely; and everyone must feel how much is unaccounted for, how much left out.

When we turn to Janet's pages, we find entirely other forms of thought employed. Oscillations of the level of mental energy, differences of tension, splittings of consciousness, sentiments of insufficiency and of unreality, substitutions, agitations and anxieties, depersonalizations—such are the elementary conceptions which the total view of his patient's life imposes on this clinical observer. They have little or nothing to do with the usual laboratory categories. Ask a scientific psychologist to predict what symptoms a patient must have when his 'supply of mental energy' diminishes, and he can utter only the word 'fatigue.' He could never predict such consequences as Janet subsumes under his one term 'psychasthenia'—the most bizarre obsessions and agitations, the most complete distortions of the relation between the self and the world.

I do not vouch for Janet's conceptions being valid, and I do not say that the two ways of looking at the mind contradict each other or are mutually incongruous; I simply say that they are incongru*ent*. Each covers so little of our total mental life that they do not even interfere or jostle. Meanwhile the clinical conceptions, though they may be vaguer than the analytic ones, are certainly more adequate, give the concreter picture of the way the whole mind works, and are of far more urgent practical importance. So the 'physician's attitude,' the 'functional psychology,' is assuredly the thing most worthy of general study to-day.

I wish to spend this hour on one conception of functional psychology, a conception never once mentioned or heard of in laboratory circles, but used perhaps more than any other by common, practical men —I mean the conception of the *amount of energy available* for running one's mental and moral operations by. Practically every one knows in his own person the difference between the days when the tide of this energy is high in him and those when it is low, though no one knows exactly what reality the term energy covers when used here, or what its tides, tensions, and levels are in themselves. This vagueness is probably the reason why our scientific psychologists ignore the conception altogether. It undoubtedly connects itself with the energies of the nervous system, but it presents fluctuations that can not easily be translated into neural terms. It offers itself as the notion of a quantity, but its ebbs and floods produce extraordinary qualitative results. To have its level raised is the most important thing that can happen to a man, yet in all my reading I know of no single page or paragraph of a scientific psychology book in which it receives mention—the psychologists have left it to be treated by the moralists and mind-curers and doctors exclusively.

Every one is familiar with the phenomenon of feeling more or less alive on different days. Every one knows on any given day that there are energies slumbering in him which the incitements of that day do not call forth, but which he might display if these were greater. Most of us feel as if we lived habitually with a sort of cloud weighing on us, below our highest notch of clearness in discernment, sureness in reasoning, or firmness in deciding. Compared with what we ought to be, we are only half awake. Our fires are damped, our drafts are checked. We

are making use of only a small part of our possible mental and physical resources. In some persons this sense of being cut off from their rightful resources is extreme, and we then get the formidable neurasthenic and psychasthenic conditions, with life grown into one tissue of impossibilities, that the medical books describe.

Part of the imperfect vitality under which we labor can be explained by scientific psychology. It is the result of the inhibition exerted by one part of our ideas on other parts. Conscience makes cowards of us all. Social conventions prevent us from telling the truth after the fashion of the heroes and heroines of Bernard Shaw. Our scientific respectability keeps us from exercising the mystical portions of our nature freely. If we are doctors, our mind-cure sympathies, if we are mind-curists, our medical sympathies, are tied up. We all know persons who are models of excellence, but who belong to the extreme philistine type of mind. So deadly is their intellectual respectability that we can't converse about certain subjects at all, can't let our minds play over them, can't even mention them in their presence. I have numbered among my dearest friends persons thus inhibited intellectually, with whom I would gladly have been able to talk freely about certain interests of mine, certain authors, say, as Bernard Shaw, Chesterton, Edward Carpenter, H. G. Wells, but it wouldn't do, it made them too uncomfortable, they wouldn't play, I had to be silent. An intellect thus tied down by literality and decorum makes on one the same sort of impression that an able-bodied man would who should habituate himself to do his work with only one of his fingers, locking up the rest of his organism and leaving it unused.

In few of us are functions not tied-up by the exercise of other functions. G. T. Fechner is an extraordinary exception that proves the rule. He could use his mystical faculties while being scientific. He could be both critically keen and devout. Few scientific men can pray, I imagine. Few can carry on any living commerce with 'God.' Yet many of us are well aware how much freer in many directions and abler our lives would be, were such important forms of energizing not sealed up. There are in everyone potential forms of activity that actually are shunted out from use.

The existence of reservoirs of energy that habitually are not tapped is most familiar to us in the phenomenon of 'second wind.' Ordinarily we stop when we meet the first effective layer, so to call it, of fatigue. We have then walked, played, or worked 'enough,' and desist. That amount of fatigue is an efficacious obstruction, on this side of which our usual life is cast. But if an unusual necessity forces us to press onward, a surprising thing occurs. The fatigue gets worse up to a certain critical point, when gradually or suddenly it passes away, and we are fresher than before. We have evidently tapped a level of new energy, masked until then by the fatigue-obstacle usually obeyed. There may be layer after layer of this experience. A third and a fourth 'wind' may supervene. Mental activity shows the phenomenon as well as physical, and in exceptional cases we may find, beyond the very extremity of fatigue-distress, amounts of ease and power that we never dreamed ourselves to own, sources of strength habitually not taxed at all, because habitually we never push through the obstruction, never pass those early critical points.

When we do pass, what makes us do so? Either some unusual stimulus fills us with emotional excitement, or some unusual idea of necessity induces us to make an extra effort of will. *Excitements, ideas,*

and efforts, in a word, are what carry us over the dam.

In those hyperesthetic conditions which chronic invalidism so often brings in its train, the dam has changed its normal place. The pain-threshold is abnormally near. The slightest functional exercise gives a distress which the patient yields to and stops. In such cases of 'habit-neurosis' a new range of power often comes in consequence of the bullying-treatment, of efforts which the doctor obliges the patient, against his will, to make. First comes the very extremity of distress, then follows unexpected relief. There seems no doubt that we are each and all of us to some extent victims of habit-neurosis. We have to admit the wider potential range and the habitually narrow actual use. We live subject to inhibition by degrees of fatigue which we have come only from habit to obey. Most of us may learn to push the barrier farther off, and to live in perfect comfort on much higher levels of power.

Country people and city people, as a class, illustrate this difference. The rapid rate of life, the number of decisions in an hour, the many things to keep account of, in a busy city man's or woman's life, seem monstrous to a country brother. He doesn't see how we live at all. But settle him in town; and in a year or two, if not too old, he will have trained himself to keep the pace as well as any of us, getting more out of himself in any week then he ever did in ten weeks at home. The physiologists show how one can be in nutritive equilibrium, neither losing nor gaining weight, on astonishingly different quantities of food. So one can be in what I might call 'efficiency-equilibrium' (neither gaining nor losing power when once the equilibrium is reached), on astonishingly different quantities of work, no matter in what dimension the work may be measured. It may be physical work, intellectual work, moral work, or spiritual work.

Of course there are limits: the trees don't grow into the sky. But the plain fact remains that men the world over possess amounts of resource, which only very exceptional individuals push to their extremes of use.

The excitements that carry us over the usually effective dam are most often the classic emotional ones, love, anger, crowd-contagion, or despair. Life's vicissitudes bring them in abundance. A new position of responsibility, if it do not crush a man, will often, nay, one may say, will usually, show him to be a far stronger creature than was supposed. Even here we are witnessing (some of us admiring, some deploring —I must class myself as admiring) the dynamogenic effects of a very exalted political office upon the energies of an individual who had already manifested a healthy amount of energy before the office came.

Mr. Sydney Olivier has given us a fine fable of the dynamogenic effects of love in a late story called 'The Empire Builder,' in the *Contemporary Review* for May, 1905. A young naval officer falls in love at sight with a missionary's daughter on a lost island, which his ship accidentally touches. From that day onward he must see her again; and he so moves Heaven and earth and the Colonial Office and the Admiralty to get sent there once more, that the island finally is annexed to the empire in consequence of the various fusses he is led to make. People must have been appalled lately in San Francisco to find the stores of bottled up energy and endurance they possessed.

Wars, of course, and shipwrecks, are the great revealers of what men and women are able to do and bear. Cromwell's and Grant's careers are the stock examples of how war will wake a man up. I owe to

Professor Norton's kindness the permission to read to you part of a letter from Colonel Baird-Smith, written shortly after the six weeks' siege of Delhi in 1857, for the victorious issue of which that excellent officer was chiefly to be thanked. He writes as follows:

* * * My poor wife had some reason to think that war and disease between them had left very little of a husband to take under nursing when she got him again. An attack of camp-scurvy had filled my mouth with sores, shaken every joint in my body, and covered me all over with sores and livid spots so that I was marvelously unlovely to look upon. A smart knock on the ankle-joint from the splinter of a shell that burst in my face, in itself a mere bagatelle of a wound, had been of necessity neglected under the pressing and incessant calls upon me, and had grown worse and worse till the whole foot below the ankle became a black mass and seemed to threaten mortification. I insisted however on being allowed to use it till the place was taken, mortification or no; and though the pain was sometimes horrible, I carried my point and kept up to the last. On the day after the assault I had an unlucky fall on some bad ground, and it was an open question for a day or two whether I hadn't broken my arm at the elbow. Fortunately it turned out to be only a very severe sprain, but I am still conscious of the wrench it gave me. To crown the whole pleasant catalogue, I was worn to a shadow by a constant diarrhœa, and consumed as much opium as would have done credit to my father-in-law.[2] However, thank God I have a good share of Tapleyism in me and come out strong under difficulties. I think I may confidently say that no man ever saw me out of heart, or ever heard one croaking word from me even when our prospects were gloomiest. We were sadly scourged by the cholera and it was almost appalling to me to find that out of twenty-seven officers present, I could only muster fifteen for the operations of the attack. However, it was done, and after it was done came the collapse. Don't be horrified when I tell you that for the whole of the actual siege, and in truth for some little time before, I almost lived on brandy. Appetite for food I had none, but I forced myself to eat just sufficient to sustain life, and I had an incessant craving for brandy as the strongest stimulant I could get. Strange to say, I was quite unconscious of its affecting me in the slightest degree. *The excitement of the work was so great that no lesser one seemed to have any chance against it, and I certainly never found my intellect clearer or my nerves stronger in my life.* It was only my wretched body that was weak, and the moment the real work was done by our becoming complete masters of Delhi, I broke down without delay and discovered that if I wished to live I must continue no longer the system that had kept me up until the crisis was past. With it passed away as if in a moment all desire to stimulate, and a perfect loathing of my late staff of life took possession of me.

Such experiences show how profound is the alteration in the manner in which, under excitement, our organism will sometimes perform its physiological work. The metabolisms become different when the reserves have to be used, and for weeks and months the deeper use may go on.

Morbid cases, here as elsewhere, lay the normal machinery bare. In the first number of Dr. Morton Prince's *Journal of Abnormal Psychology,* Dr. Janet has discussed five cases of morbid impulse, with an explanation that is precious for my present point of view. One is a girl who eats, eats, eats, all day. Another walks, walks, walks, and gets her food from an automobile that escorts her. Another is a dipsomaniac. A fourth pulls out her hair. A fifth wounds her flesh and burns her skin. Hitherto such freaks of impulse have received Greek names (as bulimia, dromomania, etc.) and been scientifically disposed of as 'episodic syndromata of hereditary degeneration.' But it turns out that Janet's cases are all what he calls psychasthenics, or victims of a chronic sense of weakness, torpor, lethargy, fatigue, insufficiency, impossibility, unreality, and powerlessness of will; and that in each and all of them the particular activity pursued, deleterious though it be, has the temporary result of raising the sense of vitality and making the patient feel alive again. These things reanimate; they

[2] [Thomas De Quincey.—W. J.]

would reanimate *us;* but it happens that in each patient the particular freak-activity chosen is the only thing that does reanimate; and therein lies the morbid state. The way to treat such persons is to discover to them more usual and useful ways of throwing their stores of vital energy into gear.

Colonel Baird-Smith, needing to draw on altogether extraordinary stores of energy, found that brandy and opium were ways of throwing them into gear.

Such cases are humanly typical. We are all to some degree oppressed, unfree. We don't come to our own. It is there, but we don't get at it. The threshold must be made to shift. Then many of us find that an excentric activity—a 'spree,' say—relieves. There is no doubt that to some men sprees and excesses of almost any kind are medicinal, temporarily at any rate, in spite of what the moralists and doctors say.

But when the normal tasks and stimulations of life don't put a man's deeper levels of energy on tap, and he requires distinctly deleterious excitements, his constitution verges on the abnormal. The normal opener of deeper and deeper levels of energy is the will. The difficulty is to use it; to make the effort which the word volition implies. But if we *do* make it (or if a god, though he were only the god Chance, makes it through us), it will act dynamogenically on us for a month. It is notorious that a single successful effort of moral volition, such as saying 'no' to some habitual temptation, or performing some courageous act, will launch a man on a higher level of energy for days and weeks, will give him a new range of power.

The emotions and excitements due to usual situations are the usual inciters of the will. But these act discontinuously; and in the intervals the shallower levels of life tend to close in and shut us off. Accordingly the best practical knowers of the human soul have invented the thing known as methodical ascetic discipline to keep the deeper levels constantly in reach. Beginning with easy tasks, passing to harder ones, and exercising day by day, it is, I believe, admitted that disciples of asceticism can reach very high levels of freedom and power of will.

Ignatius Loyola's spiritual exercises must have produced this result in innumerable devotees. But the most venerable ascetic system, and the one whose results have the most voluminous experimental corroboration is undoubtedly the Yoga system in Hindostan. From time immemorial, by Hatha Yoga, Raja Yoga, Karma Yoga, or whatever code of practise it might be, Hindu aspirants to perfection have trained themselves, month in and out, for years. The result claimed, and certainly in many cases accorded by impartial judges, is strength of character, personal power, unshakability of soul. But it is not easy to disentangle fact from tradition in Hindu affairs. So I am glad to have a European friend who has submitted to Hatha Yoga training, and whose account of the results I am privileged to quote. I think you will appreciate the light it throws on the question of our unused reservoirs of power.

My friend is an extraordinarily gifted man, both morally and intellectually, but has an instable nervous system, and for many years has lived in a circular process of alternate lethargy and over-animation: something like three weeks of extreme activity, and then a week of prostration in bed. An unpromising condition, which the best specialists in Europe had failed to relieve; so he tried Hatha Yoga, partly out of curiosity, and partly with a sort of desperate hope. What follows is a short ex-

tract from a letter sixty pages long which he addressed me a year ago:

Thus I decided to follow Vivekananda's advice: "Practise hard: whether you live or die by it doesn't matter." My improvised chela and I began with starvation. I do not know whether you did try it ever * * * but voluntary starvation is very different from involuntary, and implies more temptations. We reduced first our meals to twice a day and then to once a day. The best authorities agree that in order to control the body fasting is essential, and even in the Gospel the worst spirits are said to obey only those who fast and pray. We reduced very much the amount of food, disregarding chemical theories about the need of albumen, sometimes living on olive oil and bread; or on fruits alone; or on milk and rice; in very small quantities—much less than I formerly ate at one meal. I began to get lighter every day, and lost 20 pounds in a few weeks; but this could not stop such a desperate undertaking * * * rather starve than live as a slave! Then besides we practised *asana* or postures, breaking almost our limbs. Try to sit down on the floor and to kiss your knees without bending them, or to join your hands on the usually unapproachable upper part of your back, or to bring the toe of your right foot to your left ear without bending the knees * * * these are easy samples of posture for a Yogi.

All the time also breathing exercises: keeping the breath in and out up to two minutes, breathing in different rhythms and positions. Also very much prayer and Roman Catholic practises combined with the Yoga, in order to leave nothing untried and to be protected against the tricks of Hindu devils! Then concentration of thought on different parts of the body, and on the processes going on within them. Exclusion of all emotions, dry logical reading, as intellectual diet, and working out logical problems. * * * I wrote a Handbook of Logic as a *Nebenprodukt* of the whole experiment.[3]

After a few weeks I broke down and had to interrupt everything, in a worse state of prostration than ever. * * * My younger chela went on unshaken by my fate; and as soon as I arose from bed I tried again, decided to fight it out, even feeling a kind of determination such as I had never felt before, a certain absolute will of victory at any price and faith in it. Whether it is my own merit or a divine grace, I can not judge for certain, but I prefer to admit the latter. I had been ill for seven years, and some people say this is a term for many punishments. However base and vile a sinner I had been, perhaps my sins were about to be forgiven, and Yoga was only an exterior opportunity, an object for concentration of will. I do not yet pretend to explain much of what I have gone through, but the fact is that since I arose from bed on August 20, no new crisis of prostration came again, and I have now the strongest conviction that no crisis will ever return. If you consider that for the past years there has not been a single month without this lethargy, you will grant that even to an outside observer four successive months of increasing health are an objective test. In this time I underwent very severe penances, reducing sleep and food and increasing the task of work and exercise. My intuition was developed by these practises: there came a sense of certainty, never known before, as to the things needed by the body and the mind, and the body came to obey like a wild horse tamed. Also the mind learned to obey, and the current of thought and feeling was shaped according to my will. I mastered sleep and hunger, and the flights of thought, and came to know a peace never known before, an inner rhythm of unison with a deeper rhythm above or beyond. Personal wishes ceased, and the consciousness of being the instrument of a superior power arose. A calm certainty of indubitable success in every undertaking imparts great and real power. I often guessed the thoughts of my companion * * * we observed generally the greatest isolation and silence. We both felt an unspeakable joy in the simplest natural impressions, light, air, landscape, any kind of simplest food; and above everything in rhythmical respiration, which produces a state of mind without thought or feeling, and still very intense, indescribable.

These results began to be more evident in the fourth month of uninterrupted training. We felt quite happy, never tired, sleeping only from 8 P.M. to midnight, and rising with joy from our sleep to another day's work of study and exercise. * * *

I am now in Palermo, and have had to neglect the exercises in the last few days, but I feel as fresh as if I were in full training and see the sunny side of all things. I am not in a hurry, rushing to complete ——.

And here my friend mentions a certain life-work of his own about which I had

[3] This handbook was published last March.—W. J.

better be silent. He goes on to analyze the exercises and their effects in an extremely practical way, but at too great length for me to entertain you with. Repetition, alteration, periodicity, parallelism (or the association of the idea of some desirable vital or spiritual effect with each movement), etc., are laws which he deems highly important. "I am sure," he continues, "that everybody who is able to concentrate thought and will, and to eliminate superfluous emotions, sooner or later becomes a master of his body and can overcome every kind of illness. This is the truth at the bottom of all mind-cures. Our thoughts have a plastic power over the body."

You will be relieved, I doubt not, to hear my excentric correspondent here make connection at last with something you know by heart, namely, 'suggestive therapeutics.' Call his whole performance, if you like, an experiment in methodical self-suggestion. That only makes it more valuable as an illustration of what I wish to impress in as many ways as possible upon your minds, that we habitually live inside our limits of power. Suggestion, especially under hypnosis, is now universally recognized as a means, exceptionally successful in certain persons, of concentrating consciousness, and, in others, of influencing their bodies' states. It throws into gear energies of imagination, of will, and of mental influence over physiological processes, that usually lie dormant, and that can only be thrown into gear at all in chosen subjects. It is, in short, dynamogenic; and the cheapest terms in which to deal with our amateur Yogi's experience is to call it auto-suggestive.

I wrote to him that I couldn't possibly attribute any sacramental value to the particular Hatha Yoga processes, the postures, breathings, fastings and the like, and that they seemed to me but so many manners, available in his case and his chela's, but not for everybody, of breaking through the barriers which life's routine had concreted round the deeper strata of the will, and gradually bringing its unused energies into action.

He replied as follows:

You are quite right that the Yoga exercises are nothing else than a methodical way of increasing our will. Because we are unable to will at once the most difficult things, we must imagine steps leading to them. Breathing being the easiest of the bodily activities, it is very natural that it offers a good scope for exercise of will. The control of thought could be gained without breathing-discipline, but it is simply easier to control thought simultaneously with the control of breath. Anyone who can think clearly and persistently of one thing needs not breathing exercises. You are quite right that we are not using all our power and that we often learn how much we *can* only when we *must*. * * * The power that we do not use up completely can be brought [more and more] into use by what we call *faith*. Faith is like the manometer of the will, registering its pressure. If I could believe that I can levitate, I could do it. But I can not believe, and therefore I am clumsily sticking to earth. * * * Now this faith, this power of credulity, can be educated by small efforts. I can breathe at the rate of say twelve times a minute. I can easily believe that I can breathe ten times a minute. When I have accustomed myself to breathe ten times a minute, I learn to believe it will be easy to breathe six times a minute. Thus I have actually learned to breathe at the rate of once a minute. How far I shall progress I do not know. * * * The Yogi goes on in his activity in an even way, without fits of too much or too little, and he is eliminating more and more every unrest, every worry—growing into the infinite by regular training, by small additions to a task which has grown familiar. * * * But you are quite right that religious-crises, love-crises, indignation-crises, may awaken in a very short time powers similar to those reached by years of patient Yoga practise. * * * The Hindus themselves admit that Samadhi can be reached in many ways and with complete disregard of every physical training.

Allowance made for every enthusiasm and exaggeration, there can be no doubt of my

friend's regeneration—relatively, at any rate. The second letter, written six months later than the first (ten months after beginning Yoga practise, therefore), says the improvement holds good. He has undergone material trials with indifference, traveled third-class on Mediterranean steamers, and fourth-class on African trains, living with the poorest Arabs and sharing their unaccustomed food, all with equanimity. His devotion to certain interests has been put to heavy strain, and nothing is more remarkable to me than the changed moral tone with which he reports the situation. Compared with certain earlier letters, these read as if written by a different man, patient and reasonable instead of vehement, self-subordinating instead of imperious. The new tone persists in a communication received only a fortnight ago (fourteen months after beginning training)—there is, in fact, no doubt that profound modification has occurred in the running of his mental machinery. The gearing has changed, and his will is available otherwise than it was. Available without any new ideas, beliefs, or emotions, so far as I can make out, having been implanted in him. He is simply more balanced where he was more unbalanced.

You will remember that he speaks of faith, calling it a 'manometer' of the will. It sounds more natural to call our will the manometer of our faiths. Ideas set free beliefs, and the beliefs set free our wills (I use these terms with no pretension to be 'psychological'), so the will-acts register the faith-pressure within. Therefore, having considered the liberation of our stored-up energy by emotional excitements and by efforts, whether methodical or unmethodical, I must now say a word about *ideas* as our third great dynamogenic agent. Ideas contradict other ideas and keep us from believing them. An idea that thus negates a first idea may itself in turn be negated by a third idea, and the first idea may thus regain its natural influence over our belief and determine our behavior. Our philosophic and religious development proceeds thus by credulities, negations and the negating of negations.

But whether for arousing or for stopping belief, ideas may fail to be efficacious, just as a wire at one time alive with electricity, may at another time be dead. Here our insight into causes fails us, and we can only note results in general terms. In general, whether a given idea shall be a live idea, depends more on the person into whose mind it is injected than on the idea itself. The whole history of 'suggestion' opens out here. Which are the suggestive ideas for this person, and which for that? Beside the susceptibilities determined by one's education and by one's original peculiarities of character, there are lines along which men simply as men tend to be inflammable by ideas. As certain objects naturally awaken love, anger, or cupidity, so certain ideas naturally awaken the energies of loyalty, courage, endurance, or devotion. When these ideas are effective in an individual's life, their effect is often very great indeed. They may transfigure it, unlocking innumerable powers which, but for the idea, would never have come into play. 'Fatherland,' 'The Union,' 'Holy Church,' the 'Monroe Doctrine,' 'Truth,' 'Science,' 'Liberty,' Garibaldi's phrase 'Rome or Death,' etc., are so many examples of energy-releasing abstract ideas. The *social* nature of all such phrases is an essential factor of their dynamic power. They are forces of detent in situations in which no other force produces equivalent effects, and each is a force of detent only in a specific group of men.

The memory that an oath or vow has been made will nerve one to abstinences and efforts otherwise impossible: witness

the 'pledge' in the history of the temperance movement. A mere promise to his sweetheart will clean up a youth's life all over—at any rate for a time. For such effects an educated susceptibility is required. The idea of one's 'honour,' for example, unlocks energy only in those who have had the education of a gentleman, so called.

That delightful being, Prince Pueckler-Muskau, writes to his wife from England that he has invented "a sort of artificial resolution respecting things that are difficult of performance." "My device," he says, "is this: I give my word of honour most solemnly to myself to do or to leave undone this or that. I am of course extremely cautious in the use of this expedient, but when once the word is given, even though I afterwards think I have been precipitate or mistaken, I hold it to be perfectly irrevocable, whatever inconveniences I foresee likely to result. If I were capable of breaking my word after such mature consideration, I should lose all respect for myself—and what man of sense would not prefer death to such an alternative? * * * When the mysterious formula is pronounced, no alteration in my own views, nothing short of physical impossibility, must, for the welfare of my soul, alter my will. * * * I find something very satisfactory in the thought that man has the power of framing such props and weapons out of the most trivial materials, indeed out of nothing, merely by the force of his will, which thereby truly deserves the name of omnipotent."[4]

Conversions, whether they be political, scientific, philosophic, or religious, form another way in which bound energies are let loose. They unify, and put a stop to ancient mental interferences. The result is freedom, and often a great enlargement of power. A belief that thus settles upon an individual always acts as a challenge to his will. But, for the particular challenge to operate, he must be the right challeng*ee*. In religious conversions we have so fine an adjustment that the idea may be in the mind of the challengee for years before it exerts effects; and why it should do so then is often so far from obvious that the event is taken for a miracle of grace, and not a natural occurrence. Whatever it is, it may be a highwater mark of energy, in which 'noes,' once impossible, are easy, and in which a new range of 'yeses' gain the right of way.

We are just now witnessing—but our scientific education has unfitted most of us for comprehending the phenomenon—a very copious unlocking of energies by ideas, in the persons of those converts to 'New Thought,' 'Christian Science,' 'Metaphysical Healing,' or other forms of spiritual philosophy, who are so numerous among us to-day. The ideas here are healthy-minded and optimistic; and it is quite obvious that a wave of religious activity, analogous in some respects to the spread of early Christianity, Buddhism and Mohammedanism is passing over our American world. The common feature of these optimistic faiths is that they all tend to the suppression of what Mr. Horace Fletcher has termed 'fearthought.' Fearthought he defines as 'the self-suggestion of inferiority'; so that one may say that these systems all operate by the suggestion of power. And the pcwer, small or great, comes in various shapes to the individual, power, as he will tell you, not to 'mind' things that used to vex him, power to concentrate his mind, good cheer, good temper; in short, to put it mildly, a firmer, more elastic moral tone. The most genuinely saintly person I have ever known is a friend of mine now suffering from cancer of the breast. I do not assume to judge

[4] 'Tour in England, Ireland and France,' Philadelphia, 1833, p. 435.

of the wisdom or unwisdom of her disobedience to the doctors, and I cite her here solely as an example of what ideas can do. Her ideas have kept her a practically well woman for months after she should have given up and gone to bed. They have annulled all pain and weakness and given her a cheerful active life, unusually beneficent to others to whom she has afforded help.

How far the mind-cure movement is destined to extend its influence, or what intellectual modifications it may yet undergo, no one can foretell. Being a religious movement, it will certainly outstrip the purviews of its rationalist critics, such as we here may be supposed to be.

I have thus brought a pretty wide induction to bear upon my thesis, and it appears to hold good. The human individual lives usually far within his limits; he possesses powers of various sorts which he habitually fails to use. He energizes below his maximum, and he behaves below his optimum. In elementary faculty, in coordination, in power of inhibition and control, in every conceivable way, his life is contracted like the field of vision of an hysteric subject—but with less excuse, for the poor hysteric is diseased, while in the rest of us it is only an inveterate *habit*—the habit of inferiority to our full self—that is bad.

Expressed in this vague manner, everyone must admit my thesis to be true. The terms have to remain vague; for though every man of woman born knows what is meant by such phrases as having a good vital tone, a high tide of spirits, an elastic temper, as living energetically, working easily, deciding firmly, and the like, we should all be put to our trumps if asked to explain in terms of scientific psychology just what such expressions mean. We can draw some child-like psychophysical diagrams, and that is all. In physics the conception of 'energy' is perfectly defined. It is correlated with the conception of 'work.' But mental work and moral work, although we can not live without talking about them, are terms as yet hardly analyzed, and doubtless mean several heterogeneous elementary things. Our muscular work is a voluminous physical quantity, but our ideas and volitions are minute forces of release, and by 'work' here we mean the substitution of higher *kinds* for lower *kinds* of detent. Higher and lower here are qualitative terms, not translatable immediately into quantities, unless indeed they should prove to mean newer or older forms of cerebral organization, and unless newer should then prove to mean cortically more superficial, older, cortically more deep. Some anatomists, as you know, have pretended this; but it is obvious that the intuitive or popular idea of mental work, fundamental and absolutely indispensable as it is in our lives, possesses no degree whatever of scientific clearness to-day.

Here, then, is the first problem that emerges from our study. Can any one of us refine upon the conceptions of mental work and mental energy, so as later to be able to throw some definitely analytic light on what we mean by 'having a more elastic moral tone,' or by 'using higher levels of power and will'? I imagine that we may have to wait long before progress in this direction is made. The problem is too homely; one doesn't see just how to get in the electric keys and revolving drums that alone make psychology scientific to-day.

My fellow-pragmatist in Florence, G. Papini, has adopted a new conception of philosophy. He calls it the *doctrine of action* in the widest sense, the study of all human powers and means (among which latter, *truths* of every kind whatsoever figure, of course, in the first rank). From this point of view philosophy is a *prag-*

matic, comprehending, as tributary departments of itself, the old disciplines of logic, metaphysic, physic and ethic.

And here, after our first problem, two other problems burst upon our view. My belief that these two problems form a program of work well worthy of the attention of a body as learned and earnest as this audience, is, in fact, what has determined me to choose this subject, and to drag you through so many familiar facts during the hour that has sped.

The first of the two problems is *that of our powers,* the second *that of our means. of unlocking them or getting at them.* We ought somehow to get a topographic survey made of the limits of human power in every conceivable direction, something like an ophthalmologist's chart of the limits of the human field of vision; and we ought then to construct a methodical inventory of the paths of access, or keys, differing with the diverse types of individual, to the different kinds of power. This would be an absolutely concrete study, to be carried on by using historical and biographical material mainly. The limits of power must be limits that have been realized in actual persons, and the various ways of unlocking the reserves of power must have been exemplified in individual lives. Laboratory experimentation can play but a small part. Your psychologist's *Versuchsthier,* outside of hypnosis, can never be called on to tax his energies in ways as extreme as those which the emergencies of life will force on him.

So here is a program of concrete individual psychology, at which anyone in some measure may work. It is replete with interesting facts, and points to practical issues superior in importance to anything we know. I urge it therefore upon your consideration. In some shape we have all worked at it in a more or less blind and fragmentary way; yet before Papini mentioned it I had never thought of it, or heard it broached by anyone, in the generalized form of a program such as I now suggest, a program that might with proper care be made to cover the whole field of psychology, and might show us parts of it in a very fresh light.

It is just the generalizing of the problem that seems to me to make so strong an appeal. I hope that in some of you the conception may unlock unused reservoirs of investigating power.

WILLIAM JAMES

HARVARD UNIVERSITY

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE EXPANSION OF PHYSIOLOGY[1]

Looking forward into the far future, we may perhaps dimly discern the day when morphology and physiology will again join hands * * * but that day is as yet most distant.

WHEN Dr. Michael Foster, the eminent physiologist, was writing the lines quoted above, the two grand divisions of biology to which he refers seemed separated as if by a great gulf. In England and America morphology was the reigning favorite and in the higher institutions of learning physiology as such hardly existed. Both zoology and botany had come almost everywhere to mean morphology, and morphological problems were the popular themes of the day. Even in medical schools, physiology was as yet generally denied an independent existence, being commonly appended to or combined with the chair of anatomy, *i. e.,* one of morphology.

Dr. Foster was writing in the early eighties, and those who like myself can recall the conditions of biological teaching and research at that time will testify that his words were justified. It is true that a

[1] Address of the vice-president and chairman of Section K—Physiology and Experimental Medicine, New York meeting, 1906.

full professorship of physiology had been established in the Harvard Medical School in 1876, and that a brilliant young physiologist, one of Foster's own pupils, in the same year assumed the professorship of biology at Johns Hopkins. And yet I distinctly remember how strange and how full of chemistry and physics the first edition of Foster's physiology seemed as it was shown to me in 1876 by my professor of comparative anatomy, and also how two years later I studied physiology in the medical school of one of our leading universities for a whole season, under a young physician of more than ordinary ability and promise, without once seeing a demonstration—still less doing an experiment. The class simply recited,—upon so many pages of Dalton's 'Human Physiology.'

All this was the more remarkable because, according to Dr. (now President) G. Stanley Hall, physiology was at that very time "'The' German Science"—a fact stated and emphasized by Dr. Hall by the title of one of a series of contemporary essays which, re-read to-day, almost cause one to regret that the author abandoned the career of literature for that of administration. (Cf. 'Aspects of German Culture,' by G. Stanley Hall, 1881.)

Physiology, says Dr. Hall, has been characterized as just now preeminently *the* German science. This is probably true, whether it means that German physiologic methods and results are less known in other countries than those of other sciences, or that they reflect more peculiarly the national characteristics. Till Foster's text-book appeared, very little was known in England and America of German physiology, save by specialists who themselves had studied in Germany. * * * Fick terms physiology 'the highest and most fruitful generalization of the collective natural sciences.' Czermak, who devoted his wealth to building and equipping a magnificent laboratory and lecture room and his time to the end of his life to the popularization of physiology, was never weary of insisting that it should be taught in every high school. Once more, evolution in the sense of Darwin or Haeckel is far from being a finality for the physiologist. It is for him rather a morphological assumption that all animals and men belong to one family; and he defines his science with Pflüger as the chemistry and physics of living matter.

Physiology in Great Britain and America had, in fact, so far lagged behind that the publication of Foster's text-book with its revelation of some of the German physiology of the day created a real sensation. In the English-speaking countries morphology was everywhere the fashion and biologists, whether botanists or zoologists, were then, almost without an exception, morphologists. Even ten years after Foster's book appeared, Huxley, who had himself previously defined the grand divisions of biology as morphology and physiology rather than zoology and botany, speaks of zoology, in his Queen's jubilee essay on 'The Progress of Science' (1887), as if this were really morphology when he writes: "It is only in the present epoch that zoology and physiology have yielded any great aid to pathology and hygiene."

When Foster's text-book appeared descriptive zoology and embryology were already rivals in popularity, and the appearance in 1880 of Balfour's 'Comparative Embryology' made this subject for zoologists almost a passion. And yet to-day Balfour's then fascinating work seems strangely descriptive and somewhat overanxious after merely structural homologies. In his 'Introduction' Balfour, while defining embryology as covering "the anatomy and physiology of the organism during the whole period included between its first coming into being and its attainment of the adult state," is careful to add: "The present treatise deals only with the embryology of animals, and the science is moreover treated from the morphological or anatomical rather than the physiological side." So much was embryology the fashion of the day that Foster

himself issued with Balfour a well-known volume on the 'Embryology of the Chick,' and this too as a morphological, not a physiological, treatise. In view of these evidences of Balfour's preoccupation with morphological problems, it is interesting to learn from Dr. A. C. Haddon, one of his students, that Balfour always looked upon this preoccupation as temporary and that he intended to devote himself eventually to comparative physiology. Huxley, again, omitted all reference to physiology in embryology when in 1878 he defined the latter as 'an account of the anatomy of a living being at the successive periods of its existence, and of the manner in which one anatomical stage passes into the next.' And yet he incidentally recognized the equality of physiology and morphology by remarking that "geology is, as it were, the biology of our planet as a whole. In so far as it comprises the surface configuration and the inner structure of the earth it answers to morphology; in so far as it studies changes of condition and their causes it corresponds with physiology."

This supremacy of morphology continued well on into the nineties, but about ten or twelve years ago signs of a change began to appear, and no one who has observed even superficially the progress of biology during the last decade can have failed to perceive an immense and increasing interest in general and comparative physiology, accompanied by a decline of interest, relatively speaking, in pure morphology. Investigations in chemical physiology, mental physiology, embryological physiology, cytological physiology, comparative physiology, and in the general physiology of the response and the behavior of animals, have rapidly come to the front, while the field of vegetable physiology is being cultivated as never before. In its various aspects general physiology is to-day probably receiving from investigators more attention than special or mammalian (including human) physiology, and displacing in the hands of zoologists, to a remarkable extent, more strictly morphological studies of a systematic, phylogenetic or ontogenetic character.

Twenty years ago to be a zoologist meant to be a morphologist, but to-day many professors of zoology are either becoming or have already become veritable physiologists. Most of the 'experimental zoology' and 'embryology' of the present is really general physiology. So also are large parts of physiological chemistry, physiological psychology, cytology, protozoology, microbiology and bacteriology. Hygiene, climatology, experimental medicine, pharmacology, and many other modern branches of biology are also chiefly physiological rather than morphological. Foster's guarded prophecy of 1885 had an almost hopeless tone, for he put 'most distant,' and in 'the far future,' the day when 'perhaps' morphology and physiology will come together once more; and here again, for the thousandth time, prediction touching the future of science has proved to be empty and vain—for scarcely had a score of years gone by before Foster's 'most distant' day was already brightly dawning, and physiology and morphology were again 'joining hands' in experimental zoology. So far, indeed has this movement extended that even the general biologist may now claim the workers in the newer fields as immigrants into his own, pointing with pride to the breadth and depth of their work as justifying that still older idea of physiology in which it was essentially what we now call 'biology'; or even that oldest idea of all, in which physiology was the equivalent of the *ultima thule* of all these sciences, 'natural philosophy'—a term hallowed on its mathematical side by the name of Isaac Newton, and in its entirety reaching back to the pupils of Aristotle.

That the recent expansion of physiology is not really a new departure but rather a return to an older as well as a more normal condition, is another interesting fact. In 1854 there was published in London, and republished in America, the fourth edition of a thick volume of 700 pages by Dr. W. B. Carpenter, on 'Comparative Physiology,' an examination of which shows that the zoologists of that day were fully alive to many of the very problems upon which so many of our modern zoologists are engaged at the present time. The first thing that strikes us in this work is the fact that plants and animals, some high and some low in the scale of life, are equally considered, and always side by side. It is, therefore, really a treatise on general biology. The next is that physiology ordinarily so called, that is to say human physiology, is nowhere much in evidence. We also find that functions, rather than organs, are dwelt upon, and the general functions of organisms—such as alimentation, nutrition, reproduction and the liberation of heat, light and electricity—as well as the special functions of organs—absorption, circulation, respiration, and the like; the general functions always in plants as well as animals. It was therefore truly a *comparative* physiology.

Dr. Carpenter's work was published the year before I was born, but when, as a special student of biology twenty years later, I began the study of physiology, the book was never mentioned and the subject never touched upon,—both being apparently little esteemed if not actually forgotten. It was very likely this work that Foster had in mind when he wrote in the context to the passage quoted at the outset of this article: "In its more general meaning physiology was largely used of old, and is still occasionally used in popular writings, to denote an inquiry into the nature of living beings. * * * In its older sense * * * (it) corresponded to what is now called biology." Recalling the fundamental, original and epoch-making 'Handbuch' of Johannes Müller published in Germany between 1834 and 1840 (and in English from 1837) and Carpenter's textbook just referred to, on 'Comparative Physiology,' published in England and America in 1854, we are compelled to regard the present remarkable development of physiology as not merely an expansion, but also a renascence or revival—a return, as it were, to an earlier normal.

The question naturally arises, How did it happen that general and comparative physiology, after beginnings so brilliant, was virtually eclipsed from the time of Carpenter to that of Verworn—(for Claude Bernard's 'Leçons sur les phénomènes de la vie communs aux animaux et aux végétaux' published in 1878, had at the time very little general effect, and were hardly more than a succès d'estime). Why was it, we may well inquire, that the pendulum of biological research and teaching swung so far over to the morphological side, while mammalian and medical (or human) physiology rapidly advanced—at least in Germany—separated itself from anatomy, a branch of morphology, and secured for itself important and independent recognition with sustaining professorships?

To this question the answer is, I think, extremely simple—the dates mentioned, and the fact that the phenomenon was most marked in English-speaking countries, giving us the proper clue. In all probability the rapid rise of interest in general morphology and the corresponding neglect of general physiology after 1860 were alike due to the almost complete and universal absorption of biologists, and especially English-speaking biologists, in the problem of the origin of species. For getting light upon this all-important problem,

studies in morphological embryology, in comparative anatomy and in systematic zoology and botany were simply indispensable as sources of evidence bearing upon the doctrine of descent, and hence studies ontogenetic and phylogenetic, rather than physiologic, were for the time being enthusiastically and almost exclusively pursued. That medical physiology did not suffer a similar total eclipse by morphology, but rather continued to advance (at least in Germany) until in 1881 Stanley Hall could describe it as "'The' German Science," was obviously because of its technical importance to medicine. That it flourished in Germany and not in England was doubtless due partly to the persisting influence of Johannes Müller's great work and partly to the fact that the struggles over Darwinism were severest and most distracting on English soil.

Whatever the reason, it had somehow come to pass that in the eighties there was nowhere any physiology to speak of outside the medical schools (while that inside these schools was often of the poorest) and that my own generation grew up almost totally ignorant of general and comparative physiology. If the reason, as seems likely, was the rise and all-embracing influence of Darwinism we may perhaps be pardoned if Verworn's innocent remark, that the doctrine of descent has not thus far 'been fruitful in physiology' seems to some of us so far within the truth as not to touch it. But at last, when, after nearly forty years, descriptive embryology and the phylogeny of animals and plants had been well worked out, and when even the noise of the great struggle over the origin of species had mostly died away, opportunity came for that remarkable expansion of physiology which we are now witnessing and which, if I am right, is not merely an expansion, but a renascence. It is a renascence, however, not like the great period of that name in history, preceded by a dark or middle age in general knowledge, for between 1854 and 1894 a splendid development of all other sciences had taken place, theological bonds had been broken, and the freedom of speech and of research enlarged and strengthened. Chemistry and physics had wonderfully expanded and developed and were ready to shed new light upon physiological processes, so that we might say once more, and may to-day repeat with renewed confidence, what George Henry Lewes said before the eclipse —"The hope of science at the present day is to express all phenomena in terms of dynamics."

As for the importance of the revival and of the recent expansion of physiology for biology, making of the latter once more that rounded whole—*totus teres atque rotundus*—which it ought to be, it is difficult to exaggerate. If, as we believe, biology is only the chemistry and physics of living matter, and if our hope 'is to express all phenomena in terms of dynamics,' we cannot but rejoice that having accumulated within the last fifty years a vast and precious store of morphological material we may now pass on to the investigations of questions of the relation, causation and coordination of activities; of processes rather than homologies, of behavior rather than form, of mechanism rather than framework. I am informed by an excellent authority that a similar tendency is apparent in medicine itself, and that to-day the processes rather than the results of disease occupy the center of interest in pathology.

Clerk Maxwell long ago remarked concerning biology, that "sciences of this kind are rich in facts, and will be well occupied for ages to come in the coordination of these facts." Surely it is a matter for rejoicing that physiology is to-day dealing with a wider range of facts than ever be-

fore. It is no longer confined within medical schools, in which mammalian, or at least vertebrate, facts must always be of paramount importance, for, as has been shown above, zoologists and botanists in our universities and colleges are turning their attention to the behavior and activities of the lower forms of life, both plant and animal. It is, however, unfortunate that the beginner still generally finds no physiology, under that name, offered in our higher institutions of learning outside the medical schools, in which physiology is necessarily, and rightly enough, influenced to a great degree by the needs of technical students, since—as Huxley said long ago—"a medical school is a technical school: a school in which a practical profession is taught." For while physiologists have abundantly demonstrated that pure science may thrive in a technical atmosphere, still, it must always be true that in a technical school the applications of science will be most in demand, and hence most influential. Instead of reproving medical physiologists for their failure to cover the whole field we ought, however, to be grateful to them for having stuck to their guns so devotedly after all other physiologists, both general and comparative, had deserted the field and followed after morphological gods, such as embryology, homology and phylogeny. At last, however, the zoologists and botanists are returning to their own and taking up their old work. It is greatly to be hoped that some of these may eventually come to acknowledge themselves physiologists, and that, very soon, students of biology in our higher institutions of learning, whether zoologists or botanists, may have offered to them equal opportunities in general or comparative morphology and general or comparative physiology. (An excellent *résumé* of present tendencies may be found in the various addresses in Vol. V., Congress of Arts and Science, Universal Exposition. St. Louis, 1906.)

Meantime, what is most important is to realize that physiology is still and always will be one of the two grand divisions of biology; that it offers, to-day, especially in its general and its comparative divisions, a field white for the harvest and—what makes it still more inviting—one mostly unworked since the publication of the origin of species. When we realize these facts and also what a wealth of new knowledge the progress of other sciences such as chemistry and physics has placed at our disposal since 1860, it is clear that to general physiology we may probably look in the immediate future for the greatest advances in biology. We have already got from medical physiologists the broad outlines of the physiology of animal organs and from plant physiologists of the organs of plants; we are getting from the experimental zoologists—and particularly the embryologists and cytologists—the physiology of animal cells—including various protoplasms. Our next great advance must come in the physiology of organisms as wholes, and that not merely of the lower organisms, but of the higher also. In this direction studies on nutrition are already beginning to tell, and epidemiology has much to teach. When climatology—a science of rare possibilities—and the numerous divisions of public hygiene shall have 'coordinated their facts,' we shall have at least the groundwork of a complete physiology of the higher organisms.

The discussion which is to follow—a discussion possible indeed only after morphology had cleared the way—should afford in a consideration of the actions and reactions between parasitic protozoa and mankind, an excellent example of the broader general physiology of to-day and to-morrow.

WILLIAM T. SEDGWICK
MASSACHUSETTS INSTITUTE
OF TECHNOLOGY

THE AMERICAN FEDERATION OF TEACHERS OF THE MATHEMATICAL AND THE NATURAL SCIENCES

In accordance with a call, issued by joint action of a committee of the American Society of Teachers of Mathematics and the Natural Sciences and one of the Central Association of Science and Mathematics Teachers, a meeting of delegates of a number of associations was held in New York on December 27, 1906, for the purpose of discussing the formation of a federation of associations of teachers of science and mathematics. A roll of the meeting was taken, and it was found that there were present 27 delegates, representing seven associations, as follows: The Association of Teachers of Mathematics of the Middle States and Maryland, 9 delegates; the New York State Science Teachers' Association (Mathematics Section), 6 delegates; the Central Association of Science and Mathematics Teachers, 5 delegates; the Association of the Teachers of Mathematics of New England, 3 delegates; the Association of the Teachers of Physics of Washington City, 2 delegates; the Missouri Society of Teachers of Mathematics and Science, 1 delegate; the New Jersey State Science Teachers' Association, 1 delegate.

Professor T. S. Fiske, of the Association of Teachers of Mathematics of the Middle States and Maryland, was elected chairman of the meeting, and Professor C. R. Mann, of the Central Association of Science and Mathematics Teachers, was made secretary.

After some preliminary discussion, it was, on motion duly seconded, unanimously voted:

That it is recommended that there be formed, by the various associations of teachers of science and of mathematics, an 'American Federation of Teachers of the Mathematical and the Natural Sciences.'

The question of the form of the organization was then taken up. Two different forms were proposed: one, that of a single society of teachers of mathematics and the mathematical sciences, the membership to be limited to associations that publish literature and reports; the other, a rather loose federation of all associations of teachers of either mathematical or natural sciences, the membership being limited to associations that have more than fifty members. The first of these forms was that adopted by the American Society of Teachers of Mathematics and the Natural Sciences at the conference held at Asbury Park in 1905. The latter form was proposed by the Central Association of Science and Mathematics Teachers.

In the discussion of this question, the latter form of organization was shown to be less formal and more flexible and to interfere less with the individual activities of the associations. Because this form of federation appeared to furnish the necessary basis for a first step toward a more complete organization, and because it was considered advisable that associations not represented at the meeting should have a voice in the final decision, it was, on motion duly seconded, unanimously voted:

That the form of organization proposed by the Central Association of Science and Mathematics Teachers in the printed circular issued by them be tentatively adopted for the coming year, the final form of organization to be decided at the next meeting.

No officers were elected; but an executive committee, which should look after the formation and development of the Federation pending permanent organization, was elected as follows: T. S. Fiske, Columbia University, *chairman;* C. R. Mann, University of Chicago, *secretary-treasurer;* H. W. Tyler, Massachusetts Institute of Technology; R. E. Dodge, Teachers College, New York; F. N. Peters, Kansas City High School.

On motion duly seconded, it was voted: That this executive committee have power to fill vacancies and to add to its membership by unanimous vote.

On motion, the meeting adjourned, subject to the call of the executive committee.

C. R. MANN,
Secretary

SCIENTIFIC BOOKS

Animal Micrology: Practical Exercises in Microscopical Methods. By MICHAEL F. GUYER, Ph.D., Professor of Zoology in the University of Cincinnati. Chicago, The University of Chicago Press. 1906.

This little book of 240 pages is devoted to a concise, eminently practical and well-classified treatment of the methods and 'tricks' of convenience fundamental to modern microscopic study. While it is intended primarily for the beginner, its consultation will be found profitable to all of us who have to suffer the trials and time-consuming details of microscopical technique. The author's years of experience in giving instruction in general zoology and microscopic anatomy, combined with his marked ingenuity in mechanical and chemical manipulation, has resulted, not only in a well-grounded knowledge of the fundamental principles upon which depend the successful application of the various methods, but also in the devising and proving of numerous little simplifications and time-saving 'short-cuts' of procedure which will be appreciated by the advanced student and investigator as well. On the other hand, the treatment is expressly detailed enough for the piloting of the beginner safely through the various methods, and, methods for given purposes being chosen for him, he is saved from the bewildering maze of the superfluity of present-day methods. The student is told definitely what to do with his material, what method to apply for a given result, how to proceed step by step, and is given either the positive or the most probable reasons for the various steps.

Unlike other books of a similar nature, Professor Guyer's book is not confined to a single branch of the subject, such as histological or embryological methods exclusively, nor does it attempt to include material to the extent of making it bulky and unwieldy. However, it embraces the methods necessary in practically the whole field of the more usual biological courses and is thus purposely adapted for those combination courses given in high schools and colleges, which begin, preliminarily, with the simpler forms of life and pass to the consideration of the tissues and organology of the higher forms, giving some attention to embryology, and neurology as such.

The book is divided into seventeen chapters and five appendices. Beginning with a useful list of the apparatus and supplies usually required, the former well illustrated, the arrangement thence consists of a general statement concerning methods and the needs for them, followed by the procedures for 'killing' and 'fixing,' a description of the simple methods of sectioning, the methods of imbedding and sectioning in paraffin and celloidin, the processes of staining and 'mounting,' the method of frozen sections, the methods involving the precipitation of metallic substances for special differentiations, methods for the isolation of elements by 'tricks of teasing' and use of dissociating fluids, continuing with methods for the treatment of bone and other hard substances and methods for the injection of the blood- and lymph-vascular systems. Then is interpolated a chapter entitled, Objects of General Interest, in which are discussed subjects such as 'cell making' and the preparation of fluid mounts, and in which are given some ingenious devices for making 'in toto' preparations of the smaller organisms, such as water mites, transparent larvæ, small crustacea, worms, small insects and parts of insects, and for making 'opaque mounts' of such as beetles, wings of butterflies, etc. This chapter is followed by methods for the preparation and study of blood and a chapter dealing with the general procedures for the staining and mounting of bacteria. Chapter XVI. describes some of the methods necessary in the study of embryology, including technique for whole mounts and for the measuring and serial sectioning of embryos, special applica-

tions for the chick, teleosts, amphibia and mammalia, and directions for the artificial fecundation and study of the early cleavage of forms permitting it. Chapter XVII. gives the two most generally used methods for the reconstruction of specimens from sections, namely, reconstruction with wax plates and geometrical reconstruction.

Memoranda are given at the end of each chapter and these are often more interesting to one familiar with the general working of the methods than the procedure for the methods to which the chapter is devoted, for it is in these memoranda that various adaptive modifications of the methods are given, valuable suggestions as to technique in dissection, the choice of tissues for the purpose in mind, the construction and manipulation of the necessary apparatus, the selection and making up of the reagents required, and, equally important, suggestions as to the most probable causes of failure and the steps in procedure at which special care should be exercised. The substance of the memoranda might, less wisely, have been included in the body of the chapters, but, as the author states, they are, instead, appended to each chapter in order to supply additional information more or less pertinent without obscuring the main features of the methods under consideration.

Of the appendices, the first is devoted to the construction and discussion of the microscope and the optical principles involved in its use, with directions for its manipulation and an alphabetically arranged list of the more commonly used microscopical terms and appliances. In the second appendix is given a series of formulæ well chosen as representing some of the more efficient and frequently used reagents, including fixing and hardening fluids, stains, indifferent fluids, dissociating and decalcifying fluids. After each fluid is noted its peculiar advantages and some of the tissues and purposes to which it is best adapted. The third appendix is a tabulation of a large number of tissues and organs arranged alphabetically in systems with concise directions in appropriate columns for the obtaining, fixing and after-treatment of each; while the fourth appendix is especially devoted to directions for the collection and preparation of the various materials necessary for a general course in zoology. The last appendix consists merely of four conveniently constructed reference tables of equivalent weights and measures.

The book is amply illustrated as to the different apparatus required and, while one might criticize the prominence with which the names of firms making the apparatus frequently stand out in the cuts, a little advertising is allowable in exchange for the excellence of the cuts used.

On the whole, the extended scope of the book, together with its conciseness of construction and reasonable price, renders it highly commendable, and, in my opinion, it will be found useful to a larger number of people than any other book of its kind at present in existence in English. Since each experienced worker in microscopical technique has his own devices of manipulation which work best for him, there are, of course, some instances in which the author's 'steps' and 'tricks' may be disputed as being the most efficient. Professor Guyer modestly recognizes this. However, with such workers, the book will be found full of helpful suggestions, new to many. The general student will find that all the methods recommended will yield good results when the directions are intelligently followed, and the fact that the author has striven to make the book thoroughly practical: 'to omit everything that is not essential, and, above all, to give definite statements about things,' has resulted in a much-desired brevity of treatment and obviation of bulk.

Irving Hardesty

The University of California

Recent Progress in the Study of Variation, Heredity and Evolution. By Robert Heath Lock. Pp. 299. London, John Murray. 1906.

At this time when the systematic botanists and zoologists differ greatly in regard to their large number of tissues and organs arranged reader will find the book under review a most useful help in arriving at sensible conclusions. Mr. Lock is well fitted to discuss the subjects

announced in the lengthy title of his book. He is intimately acquainted with the recent work in breeding, and has, himself, made some valuable contributions to knowledge. As a student at the Royal Botanical Gardens in Peradeniya, Ceylon, and later in Cambridge he has worked with Indian corn and with peas.

The book begins with an introduction in which are briefly discussed: Linnæan species, Jordan's species, variation, mutation, discontinuity of species, the work of Mendel and evolution theories. Later chapters are largely given to a fuller discussion of the topics here introduced. The first half of the book is rather elementary, intended presumably for the general reader. Natural selection, evidences of evolution and 'biometry' are treated in detail. It must not be supposed that the treatment of these topics is purely perfunctory. Even in the driest parts of the work there are sharp and valuable criticisms of the theories of the day. The author pays his respects to the theories of 'protective resemblances,' 'mimicry' and 'inheritance of acquired characters.' He shows the inadequacy of natural selection for the origin of species and prepares the reader for the subject evidently most dear to his own heart—'Mendelism.'

In describing the operation of Mendel's Law our author is at his best. He makes clear some things not generally understood in regard to the position of the 'Mendelians.' Thus (p. 180) he says: "dominance is by no means an universal phenomenon. * * * In a considerable number of instances the heterozygote is found to exhibit an appearance which is more or less intermediate between the types of character shown by the parents."

On page 205 it is shown that new forms arising in the midst of an old-established species need not be 'swamped' by intercrossing. A chapter on 'Recent Cytology' is chiefly an elementary account of the cell, but some discussion is given of the probable relation between chromosomes and Mendelian characters. Weismann's views and those of the 'Mendelians' are contrasted (pp. 261–262). The discussion of the alternating generations of plants as the 'x-generation' and '$2x$-generation' (p. 270 et seq.) will interest some readers, while his remarks on the improvement of the breed in the human race will not be taken more seriously than intended by the author.

The book has few glaring faults. There is no bibliography. This is most unfortunate, since the work is so well calculated to introduce college students to the problems of heredity and evolution. Certainly a few of the more useful works might have been named. On page 92 Davenport's 'Statistical Methods' is referred to as 'Structural Methods.' Dr. MacDougal is called Macdougal (p. 139). Perhaps 'nitch' (p. 286) is not a misprint for 'niche' but an example of reformed spelling. A lack of subheadings makes the book less easily used for reference than it should be.

FRANCIS RAMALEY

UNIVERSITY OF COLORADO

SCIENTIFIC JOURNALS AND ARTICLES

The opening (January) number of volume 8 of the *Transactions of the American Mathematical Society* contains the following papers:

G. A. MILLER: 'Generalization of the groups of genus zero.'

F. MORLEY: 'On reflexive geometry.'

G. A. MILLER: 'The groups in which every subgroup is either abelian or hamiltonian.'

H. F. BLICHFELDT: 'On modular groups isomorphic with a given linear group.'

W. E. STORY: 'Denumerants of double differentiants.'

A. RANUM: 'The groups of classes of congruent matrices, with application to the group of isomorphisms of any abelian group.'

CLARA E. SMITH: 'A theorem of Abel and its application to the development of a function in terms of Bessel's functions.'

W. B. FITE: 'Irreducible linear homogeneous groups whose orders are powers of a prime.'

L. P. EISENHART: 'Applicable surfaces with asymptotic lines of one surface corresponding to a conjugate system of another.'

THE December number (volume 13, number 3) of the *Bulletin of the American Mathematical Society* contains: Report of the September Meeting of the San Francisco Section, by W. A. Manning; 'Projective Differential

Geometry,' by E. J. Wilczynski; 'On Loci the Coordinates of whose Points are Abelian Functions of Three Parameters,' by J. I. Hutchinson; 'Associated Configurations of the Cayley-Veronese Class,' by W. B. Carver; 'Von Helmholtz,' by E. B. Wilson; 'Pierpont's Theory of Functions' (Review of Pierpont's Theory of Functions of Real Variables, Volume I.), by G. A. Bliss; 'The Mathematical Tripos for 1906,' by Virgil Snyder; Shorter Notices: Simon's Ueber die Entwicklung der Elementar-Geometrie im XIX. Jahrhundert and Simon's Methodik der Elementaren Arithmetik in Verbindung mit Algebraischer Analysis, by D. E. Smith; Randall's Elements of Descriptive Geometry and Ferris's Elements of Descriptive Geometry, by L. I. Hewes; 'Notes'; 'New Publications.'

The January number contains: Report of the October Meeting of the American Mathematical Society, by F. N. Cole; Report of the Stuttgart Meeting of the Deutsche Mathematiker-Vereinigung, by A. B. Frizell; 'A New Approximate Construction for π,' by George Peirce; 'Note on Conjugate Potentials,' by O. D. Kellogg; 'Groups of Order p^m Containing Exactly $p+1$ Abelian Subgroups of Order p^{m-1},' by G. A. Miller; 'Note on Systems of In- and Circumscribed Polygons,' by Miss S. F. Richardson; 'Hermite's Works' (Review of Picard's Oeuvres de Charles Hermite, Volume I.) by James Pierpont; 'Projective Differential Geometry' (Review of Wilczynski's Projective Differential Geometry of Curves and Ruled Surfaces), by Virgil Snyder; Shorter Notices: Nielsen's Handbuch der Theorie der Cylinderfunktionen, by F. H. Safford; Mach's Space and Geometry in the Light of Physiological, Psychological and Physical Inquiry, by C. J. Keyser; 'Notes'; 'New Publications.'

The February number contains: Report of the Preliminary Meeting of the Southwestern Section, by A. S. Chessin; 'Selected Topics in the Theory of Boundary Value Problems of Differential Equations,' by Max Mason; 'Note on Fourier's Constants,' by E. H. Moore; 'On the Minimum Number of Operators Whose Orders Exceed Two in any Finite Group,' by G. A. Miller; 'Note on the Orientation of a Secant,' by L. D. Ames; 'On Euler's ϕ-Function,' by R. D. Carmichael; Shorter Notices: Muir's Theory of Determinants, Revised Edition, by G. A. Miller; Schmall's First Course in Analytical Geometry, by Miss E. B. Cowley; Hefter and Koehler's Lehrbuch der analytischen Geometrie, by Miss E. B. Cowley; Teixeira's Tratado de las Curvas Especiales Notables, by C. H. Sisam; The Scientific Papers of J. Willard Gibbs, by E. B. Wilson; Gerland's Leibnizens Nachgelassene Schriften physikalischen, mechanischen und technischen Inhalts, by Florian Cajori; 'Notes'; 'New Publications.'

SOCIETIES AND ACADEMIES

THE AMERICAN CHEMICAL SOCIETY. NEW YORK SECTION

The fourth regular meeting of the session of 1906–7 was held at the Chemists' Club, 108 West 55th Street, on February 8.

The following papers were presented:

The Alkylation of 4-Quinazolones: M. T. Bogert and H. A. Seil.

The Synthesis of Naphthotetrazines from p. Diamino Terephthalic Compounds: J. M. Nelson and M. T. Bogert.

Note on the Use of Ultra-violet Light in Concentrating Willemite: G. C. Stone.

Ultra-violet light is used during the concentration of Willemite to determine when the tailings are free from the ore, the degree of fluorescence giving a good indication of the amount of ore present in the sample under examination. This method of analysis was illustrated on the lecture table by subjecting samples of Willemite which had been more or less completely extracted to the action of light rich in ultra-violet rays. Of the several samples of tailings examined, some showed no signs of fluorescence, while others showed the presence of sufficient ore to make it worth while to rework them.

The Determination of Sulphurous Acid in Gelatin: A Manufacturer's Position with Regard to the Pure Food Act: Jerome Alexander.

In the determination of sulphurous acid by the official method, the sample is distilled with dilute phosphoric acid in a current of carbon dioxide, the distillate collected in standard iodine solution, the excess of which is titrated with sodium thiosulphate.

The method more generally used consists in collecting the sulphurous acid in iodine solution, boiling off the iodine and precipitating the resulting sulphuric acid as barium sulphate. This method gives lower results than the official method, indicating that substances other than sulphurous acid distil over and reduce the iodine.

Samples of gelatin were analyzed by both the above methods and by different chemists. The results obtained varied considerably between the methods and between the chemists. Furthermore, sulphurous acid was reported where no sulphur in any form had been added to the gelatin. This last is explained by a consideration of some recent work on the determination of sulphurous acid in meat where a certain amount of sulphur is found to be normally present.

This work on gelatin makes it evident that since most of the food standards are based on the presence or absence of definite percentages of certain elements or compounds, it is of vital importance that chemists compare notes and see what degree of concordance is practicable with our present tentatively official methods and agree upon some reasonable limit of tolerance to cover the differences due to personal equation and imperfect analytical methods.

The technical portion of the paper was supplemented by remarks from Dr. Leo Baekeland.

A Preliminary Communication on the Toxicity of some Aniline Dyestuffs: GUSTAVE M. MEYER.

Seven dyestuffs commonly used as food colorants were obtained from a dealer and investigated. The experiments were made on dogs and included, besides observations of the general influence of these substances, also studies of their elimination. The amount of dye used was increased daily until toxic symptoms were shown. The animals were finally chloroformed and subjected to a post-mortem examination. With one exception, the only outward symptoms induced by the feeding of these dyestuffs were such as would be brought about by the administrations of equally large amounts of any of the ordinary saline purgatives.

In a general way it may be said that, judging from the amounts given and the comparatively slight effects produced, these dyestuffs can hardly be classed among virulent poisons. What symptoms they would produce if administered daily in small doses during a very long period to unhealthy animals is still undetermined.

Studies of the influence of coal-tar colors on peptic digestion in vitro indicated that all dyestuffs almost completely inhibited peptolysis when present in a concentration of 0.62 per cent. or more. Gudeman's statement that synthetic dyes have a certain food value was investigated by this method without obtaining confirmatory results. Direct conclusions as to the effect of these substances on the human body can not, however, be drawn from experiments on digestion in vitro since certain substances have opposite effects in the two cases.

Dr. Meyer's paper was ably discussed by several experts on the subject who were present. These included Messrs. Coblentz, Lieber and Schweitzer.

C. M. JOYCE,
Secretary

THE PHILOSOPHICAL SOCIETY OF WASHINGTON

THE 627th meeting was held on January 19, 1907, President Hayford in the chair. The evening was devoted to a paper by Dr. R. S. Woodward upon 'The Theory and Application of the Double Suspension Pendulum.'

THE 628th meeting was held on February 2, 1907.

Professor Newcomb read a paper on the 'Optical and Psychological Principles Involved in the Interpretation of the Markings on the Discs of the Planets.'

Two sets of principles were discussed: one optical—including all the causes which affect the formation of an image on the retina of the eye, the other psychological—including all

the causes that affect the observer's perception of the image. The first portion of the paper was concerned almost exclusively with the secondary aberration of the refracting telescope. He found that it was impossible to bring more than a small fraction, perhaps one fourth of the light emanating from a star within a radius of one tenth of a second. For this reason a black line on the planet would not be imaged as black in the telescope, but only as a gray diffused line. This effect was one that could not be cured by any arrangement or figuring of the lenses, but could be diminished by increasing the ratio of focal length to the aperture of the telescope.

The second part of the paper was devoted to what the author proposes to call 'visual inference.' This includes the process by which the eye, from the image on the retina, infers the nature of the object which produces the image. He showed at some length the extent to which this form of inference might be carried. It is based largely on experience, but in cases where this is wanting habit may take its place. One result of this process is that different people may see the same image very differently when it approaches the limit of visibility. It was shown by diagrams that broken lines under certain conditions appeared continuous, that double lines may be seen as single, or as a group of which the observer could not give the number without closer inspection. A peculiarity near the two ends of a line affected the judgment throughout the whole length of the line.

Lowell's observations of Mars were very highly spoken of as superior to all others, both in the favorable conditions under which they were made and their careful and critical character; but the general conclusion reached was that his drawings of the canals could not be accepted as certainly correct without a more complete investigation of the possible effects of visual inference in influencing the perception of the observer.

R. L. Faris,
Secretary

THE TORREY BOTANICAL CLUB

The annual meeting was called to order at the American Museum of Natural History, at 8:30 P.M., with Vice-President Burgess in the chair.

Following the presentation and acceptance of annual reports from officers and committees, the annual election resulted as follows:

President—H. H. Rusby.

Vice-Presidents—E. S. Burgess and L. M. Underwood.

Corresponding Secretary—J. K. Small.

Recording Secretary—C. Stuart Gager.

Treasurer—C. C. Curtis.

Editor—J. H. Barnhart.

Associate Editors—Philip Dowell, A. W. Evans, T. E. Hazen, M. A. Howe, W. A. Murrill, H. M. Richards, Miss A. M. Vail.

John Hendley Barnhart,
Secretary pro tem.

DISCUSSION AND CORRESPONDENCE

A SCIENCE TRUST

With the liberal appropriations by Congress for the study of the problems relating to scientific agriculture, there seems to be a growing tendency to form classes, and 'rings,' even as in the commercial and political activities of the nation. In too many cases the executive heads of the experiment stations take to themselves the credit of all that is done in their respective stations and, from the vantage ground of publicity, hamper and cripple, in many ways, the real workers in the respective fields. In many cases, of course, this injustice is unintentional; but it is none the less real. Often the true state of affairs is not realized by the offending directors; in other instances, naturally, it is not conceded.

Directors frequently assume the attitude of the political 'boss,' and attempt to 'pull the wires' in such a way that there can be no recourse for the workers except to humble themselves and 'pay court,' or to resign. They even go farther and deliberately plan to make it difficult for a worker to go from one field to a more congenial field, by throwing out insinuations as to obscure 'outs' that make a change very desirable. In other words, the 'political boss' director claims everything in sight, attempts to bully the workers into what he pleases to call 'respect for authority,' and aims to cut off any possible redress either from

local sources or from equally good situations elsewhere.

Of course, the hypothetical case here given is typical only of men of small caliber who happen to occupy directors' chairs. Nevertheless, the proposed concerted action of executive officers to prevent competition in the securing of men for certain positions, by precluding the possibility of transfer, may in many cases work injustice to the men who are, in fact, responsible for the success of every station. By this combination, and the heading off of competition, salaries are held down to a disproportionately low figure, and the inspiration of possible advancement is withdrawn.

While the importance of retaining the services of valued members of a station staff is unquestioned, and while some means of ridding the service of undesirable workers is also essential, it is equally important for the success of the work that the individual have a sense of the security of his position and that he be not subject to the whims and moods of a 'boss' who does not recognize the difference between a body of educated gentlemen, who have quite as much at stake as he himself, and a force of clerks in a mercantile establishment or a factory.

On the other hand, and every well-balanced station worker recognizes the fact, the station must move forward as a unit, and there can be but one head. With a mutual understanding, and mutual confidence between the executive head and the heads of the scientific departments, the work will move forward without the necessity of 'combining' to hold down the workers, and with much saving of friction for all parties concerned. W.

THE PRIMARY SEPTA IN RUGOSE CORALS

IN SCIENCE for August 24, 1906, and in a more recent and longer paper[1] Dr. J. E. Duerden deals in a critical and analytical way with a paper read by me before the New York Academy of Sciences and published in full in the *American Journal of Science* for February, 1906. In that paper I offered another interpretation of certain figures which Duerden had drawn of sections made through *Lophophyllum proliferum*[2] and offered evidence for the support of my interpretation. Dr. Duerden's articles call for a reply. It is made in the same spirit in which the first paper was written.

It was, perhaps, to be expected that Duerden would not agree with my interpretation; but the excellent spirit of his article is commendable. I desire to discuss the matter in an equally fair manner, without any wish to belittle any work of investigation, to ignore a profound knowledge of the particular field of discussion, or to deny the possibility of other interpretations.

I still maintain the view that the resemblance to the Zoantheæ of certain rugose coral tips in dispute is not a structural resemblance, but an apparent likeness; that the important deductions about the origin of the cardinal fossula are largely based upon this supposed resemblance, and that the number of primary septa was four. If this appears to be obstinacy in the face of strong assertions to the contrary, I desire to submit the following argument.

I am not prepared to admit that the evidence afforded by the specimens of *Streptelasma profundum* examined by me is to be considered lightly. The forms examined were undoubtedly young specimens showing all the septa and having in the tip only four primary septa. In those specimens, which were much younger than the one figured by Duerden,[3] the cup was open to the bottom, thus allowing a complete view of the septa down to the tip —the septa not having reached the center in these specimens. If a section close to the tip of these specimens were made only four septa (protosepta) would be seen. Two specimens were examined by me, both of which showed unquestionably that only four protosepta were present in the youngest stage, and Professor Grabau assures me that several others of the same kind, though less perfect, are in the collection at Columbia University.

[1] *Annals and Magazine of Natural History*, Ser. 7, Vol. XVIII., September, 1906.

[2] See 'Johns Hopkins University Circular,' January, 1902.

[3] *Biol. Bull.*, June, 1905.

Unless Duerden can prove that all of these specimens are in some way defective and can show that the base of the third pair of 'so-called primary' septa (my first pair of secondary septa) were destroyed, so that in all cases they are shorter than the other (typical) primary septa (protosepta) he can not destroy the importance of this evidence. Those specimens begin as tetrameral corals, and continue so. Unless Duerden can satisfactorily show that this tetrameral character of the youngest stage is in these specimens a result of defective silicification his argument based on sections is incomplete. Duerden must show that it is possible for a hexameral coral tip to be changed by silicification, or otherwise, to a tetrameral one in more than one example.

I am not ready to concede that mere surface views are unreliable. I hold that the inside surface view of a young form is more reliable than sections made through the tips of adult individuals. In the absence of direct proof to the point and yet without intending to beg the question I would like to inquire how one can feel sure of one's section even in forms which are not accelerated? How much more uncertainty must the probability of acceleration add!

The fact that the so-called 'primary' septa (protosepta, Duerden) under dispute have positions both in locus and in sequence that coincide with those called for by Kunth's law, and that both of these facts point to their membership among the secondary septa (metasepta, Duerden), must again be urged, and the fact that the quadrant they occupy shows evidence of acceleration must be emphasized. This acceleration is shown in the case of *Streptelasma rectum* by the fact that these quadrants generally contain more secondary septa than the other two and by the further fact that tertiary septa (exosepta, Duerden) appear next to the counter septum before they appear in the other interspaces.

It must not be forgotten that *Streptelasma profundum* is an early form and devoid of specialized characters and that the forms studied were young ones; also that the forms studied by Duerden were not only specialized, but late in time (Devonic and Carbonic).

The reassertion that the order of development is that usually ascribed to zaphrentoid corals does not affect the explanation of the alar fossulæ as instances of retardation, nor does it affect the explanation of the cardinal fossula. The explanation of the cardinal fossula as caused by the siphonoglyphe (sulcar or sulcular) is certainly ingenious, but the assumption that the gonidial groove was present was made on the previous assumption that the Tetracoralla are related to the Zoantheæ, which in turn rests upon the doubtful ground of their primary hexamerism, which was assumed from a study of sections only.

What is to be offered as the explanation of the fossula of the counter septum region? And if it is to be that offered for the cardinal fossula—the presence of a gonidial groove—what is to be advanced in support of the zoanthidian relationship, since the chief structural peculiarity of the Zoantheæ is one gonidial groove?

When the correction of cardinal and counter septum is made the cardinal septum is seen to be the small one and this fact would apparently support Duerden's view that there was a gonidial groove in this region; but I have pointed out the necessity of finding another explanation for the fossula of the counter septum or else the alternative of acknowledging that both fossulæ are caused by siphonoglyphes, which would remove these forms yet further from their hypothetical zoanthidian relatives.

To my mind the inversion of the figures counts for very little. In Duerden's criticism at this point we find nothing that affects the argument—on either side, in fact. The dorse-ventral orientation is merely arbitrary.

Such unsatisfactory definitions for *ventral* are given for designating this aspect, that for determining which to call ventral and which dorsal we must decide whether, if there be two siphonoglyphes, one of these morphological structures is more pronounced than the other, or if there be only one, whether this, in colonial types, faces the proximal end of the colony.

It must be confessed that, so far as using

the terms sulcus and sulculus for determining ventral or dorsal aspects is concerned the whole scheme is useless in so many cases that it fails of any importance. The terms are applicable only when these aspects, if they may be legitimately so called, are determined in some other way. Even though, as Haddon probably meant it should be used, the term *sulcus* be applied to that groove associated with the third pair of primary mesenteries, its use among fossil forms can hardly be said to be justified. Hence we may question the value of the term 'ventral stomodœal groove' in connection with the cardinal fossula. Indeed, we may go farther and question whether the Rugosa possessed gonidial grooves. If two grooves represented the primitive condition of the living Anthozoa we should find vestiges of a second in types with only one. The evidence, though negative, seems to point to a primitive Stomodæum without these grooves.

It is hard to see why the cardinal fossula necessitates the presence of a gonidial groove. It might have been due to the arrested development of the cardinal mesenteries (without that arrestation having been caused by a gonidial groove) combined with the other incompletely developed septa adjacent. Duerden admits, or rather independently asseverates, that such is the origin, but calls in the Siphonoglyphe to account for the small cardinal septum. What more likely than that the counter septum fossula is the result of arrested development of the corresponding mesenteries! On this wise all the fossulæ might be considered as old-age characters.

Before much can be asserted as to the order of development of the primary mesenteries in the Rugosa, specimens must be had which will indicate something about the sequence of the primary septa. It is not conclusive to reason from sections that do not inform us in this regard. In *Streptelasma profundum* the counter septum of the primary four seemed to reach farthest down into the calyx.

To summarize:

First, the argument from *L. proliferum* is not conclusive or final, since one can never be certain of having the lowest section. The statement can be extended to other forms studied by Duerden. Even *Streptelasma rectum* shows acceleration in the counter quadrants. In this form, a highly specialized type, the *tertiary* septa of the counter quadrants appear long before they do in the other quadrants, showing extreme acceleration in the counter quadrants. In an actual young specimen of *Streptelasma profundum*, in which the bottom of the corallum is shown, and the actual beginnings of the septa are visible, the four primary septa reach farther down than the secondary ones, and hence must be considered as having appeared before the secondary septa appeared. This shows the primary tetramerism of this type and is strong inferential evidence for all zaphrentoids.

Second, the inversion of figures counts for nothing. In referring to fossil forms of uncertain septal sequence and structural make-up the older terms are the more suitable.

Third, the hexameral arrangement of the septa in the Rugosa is not established, but rather is contradicted, by the evidence from the primitive members of the group. The primitive Rugosa appear to possess a pronounced quadripartite arrangement and a definite bilateral symmetry. Upon this symmetry and arrangement, by acceleration or otherwise, has been imposed a pseudohexameral arrangement, in instances, and a 'biradial symmetry.'

Fourth, this article really purposes to discuss the matter only and makes no pretense of ignoring other points of view, or of having settled the matter. C. E. GORDON

MASSACHUSETTS AGRICULTURAL COLLEGE,
AMHERST, MASS.

UNIVERSITY REGISTRATION STATISTICS

TO THE EDITOR OF SCIENCE: As the figures of the University of Chicago were not received until the article on university registration statistics (SCIENCE, December 21, 1906) was in press, it was not possible to include an accompanying notice of changes in the fall registration. The facts of the case are these:

The enrollment figures of the university as of November 10 show a slight gain, from 2,130 to

2,179, in the fall courses within the quadrangles, and a considerable loss, from 562 to 247, in the courses outside the quadrangles. Owing to the increase in the summer session figures, however, there is a gain in the grand total. The change of courses given for teachers from the center of the city to the quadrangles, which took effect this fall, has lessened the number of students in such courses, but increased the efficiency of the work.

As for individual schools, there has been a gain in the academic department, especially in men, in law and in pedagogy, while there has been a slight loss in medicine, divinity and the graduate schools. As in the case of the University of Pennsylvania, a number of students enrolled in courses for teachers have been included in the Chicago figures who would be excluded in the Columbia or Harvard figures, but the time for making more definite inquiries was too short.

The following errors should also be noted: In the list of institutions mentioned on page 794, column two, line eleven, Stanford should be inserted between Kansas and Indiana; and in line fifteen Chicago should be omitted. On page 796, column one, line eighteen, Chicago should be inserted before Harvard. In the table, the number of men in the academic department of Princeton University should be 758, instead of 755. On page 794, column one, line twenty, insert, before Missouri, 'Syracuse (48.71%).' Rudolf Tombo, Jr.

ALCOHOL FROM CACTI

To the Editor of Science: In a letter entitled 'Alcohol from Cacti,' which appeared in the *Scientific American* for December 15, the author referring to the results obtained with this plant by a California chemist, states that "from five pounds of pulp he distilled, in a crude way, more than a gallon of alcohol, which was clear in color, and burned readily with a bright, warm glow."

At the time this article appeared we were hesitating about publishing the enclosed press bulletin for fear the theoretical estimates therein given would exceed the amount which it would be possible to obtain in practise.

Cactus will not average over 10 per cent. carbohydrates, and if, as is usually estimated, this yields one half its weight of 95 per cent. alcohol, it is not clear how it would be possible to obtain one gallon of alcohol from less than 140 pounds of this plant. If, however, the chemist referred to above can distil one gallon (seven pounds) from five pounds of cactus pulp, it would be interesting to know what the strength of his product is, and whether or not it was done with the assistance of a magician's wand.

R. F. Hare

Agricultural College,
New Mexico

THE PARTHENOGENESIS OF ENCYRTUS

At the time that my recent note on 'Polyembryony and Sex-determination' was written I had not seen Silvestri's latest communication. In a brief, preliminary paper[1] he presents the results of his studies on the early stages of the development of *Encyrtus* and among other details notes the fact that, as in *Litomastix,* there is parthenogenetic development, unfertilized eggs always producing males, fertilized ones only females. The maturation and early segmentation stages studied are identical in the two types.

Wm. A. Riley

SPECIAL ARTICLES

POLARIZATION AND INTERFERENCE PHENOMENA WITH WHITE LIGHT

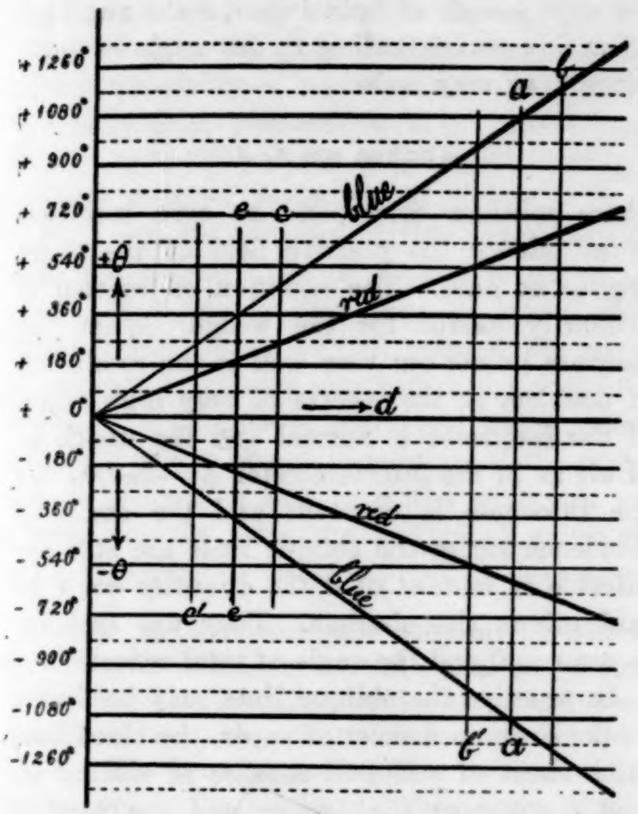
I have usually found great difficulty in endeavoring to explain the color phenomena obtained with white light in rotary polarization, in the behavior of thin plates with or without polarized light, and in interferences and diffractions generally, to an elementary class. The following diagram, therefore, which yields a large amount of information, may be of interest to the reader, although it contains nothing essentially novel. Note the occurrence of d/λ throughout.

Rotary Polarization.—If we write the rotation θ of the plane of polarization due to a thickness d of quartz cut perpendicularly to the axis,

$$\theta = \pi(1 - v'/v'') \cdot d/\lambda',$$

where v' and v'' are the velocities of right-handed and left-handed rays in the crystal

[1] Silvestri, F., 1906, 'Sviluppo dell *Ageniaspis* (*Encyrtus*) *fuscicollis* (Dalm.) Thoms.,' *Atti Acc. Lincei* (5), XV., pp. 650–658.



and λ' the wave-length or pitch of the right-handed screw, it is convenient to lay off θ in terms of d as in the chart. Two oblique lines in the positive field may then represent the amount of rotation corresponding to any two typical colors, as for instance, blue and red. The former having the smaller wave length will have the steeper slope. If the rotation is left handed, two other symmetrically oblique lines below the axis will represent this case. If the nicols are crossed the colors which fail to get through the analyzer must be rotated in multiples of π. Hence horizontal black bars intersect the whole field above and below the abscissa, to point out the colors blotted out. Colors which are rotated in odd multiples of $\pi/2$ get completely through, and these are shown by the dotted horizontal bars midway between the former. If the nicols are in parallel the black and dotted bars replace each other; or in general, if the analyzer is rotated θ to the right or to the left, the whole system of bars will move up or down, respectively, by the amount θ, as the dark band will pass through the spectrum of the right-handed plate from red to blue. The colors to be anticipated for any thickness of plate will then be given by drawing vertical lines as a, b, c, etc. Thus it is clear that only for relatively thin plates may vivid colors be expected, for here (as at c and e) there is but one or a few extinction bands in the spectrum. As the thickness increases the number of these bands increases until eventually the colors removed are practically the same as those retained and white light appears in all positions of the nicols. The spectra are channeled in the beautiful way seen with a column of quartz an inch or more long.

Furthermore, if a right and left bi-quartz be taken as in the saccharimeter the colors for the same d will be identical both for crossed and parallel nicols, but not otherwise. For the dark bands move from red to blue and from blue to red, respectively, in the two halves, on like rotation of the nicols. Moreover, if for a thickness d an additional rotation is imparted as by the sugar tube, d will pass to c in the right-handed plate but to c' in the left-handed plate. In relatively thin plates, however, the identity of color may be restored on rotating the analyzer to the right, for the dark band at c' below the abscissa moves in wave-lengths much more rapidly through the spectrum than at c above the abscissa; but the identical colors will even here not be quite the original color. For a relatively great thickness the identity can not be restored since b contains three and b' usually two extinction bands.

Finally the occurrence of successive orders of colors is suggested by the chart.

Thin Plates in Polarized Light.—The plates of thickness d are cut parallel to the axis, or the edge of the thin wedge is so ground and placed at 45 degrees to the plane of the polarizer. If we write

$$\phi = 2\pi(\mu_e - \mu_o) \cdot d/\lambda,$$

where ϕ is the phase difference of ordinary and extraordinary rays, λ the wave-length in air, μ_o and μ_e the respective indices of refraction, and if the nicols are crossed, the diagram as drawn will apply at once. Rotation of the analyzer over 90 degrees exchanges the black and dotted horizontal bars as before; but there is this essential difference, that the bars can not be moved continuously on rotating

the nicols, since the intermediate colors are elliptically polarized. Colors are white for thick plates and one may note in addition that $\mu_e - \mu_o$ and d are reciprocally related.

Interference. Diffraction.—For the case of two slits, real or virtual, at a distance c apart and r from the screen, if d is the distance of a lateral dark band from the central fringe for light of wave length λ, we may write

$$n = (2c/r) \cdot d/\lambda,$$

where odd numbers for n determine the position of the successive minima. Hence if the phase difference in which the rays meet $\phi = \pi n$ be introduced,

$$\phi = (2\pi c/r) \cdot d/\lambda$$

and the chart is applicable at once, with the understanding, however, that the dark horizontal bars now denote maxima, the dotted bars minima. This, however, is not necessary, for the bars may be moved up by inserting in one of the rays a thickness of lamina t of the refracted index μ (compensator) by the amount $2\pi(\mu - 1)t/\lambda$, until a minimum replaces the central maximum. The compensator thus has the same effect as the rotation of the nicols in the first paragraph. The chart shows the lateral extent of spectra of different orders very clearly, the first having a breadth of $d = 0$ the other breadths being proportional to the intercepts of the successive bars between the oblique lines blue and red. The overlapping of these spectra is also well shown. The spectra must be less spread out as the slopes $2\pi c/r\lambda$ are steeper. If the slit of the spectroscope be placed at a given distance, as at $d = a$, for instance, the vertical section indicates the channeled spectra which will be observed and the dark lines may be sharp enough to suffice the standardizing the spectrum.

With certain well-known changes the same remarks apply for most cases of the diffraction of white light.

Colors of Thin Films. Ordinary Light.—If d denote the thickness of the film or a given section of the wedge of refractive index μ, λ the wave length of light in air, r the angle of refraction corresponding to the angle of incidence i we may write

$$\phi = 2\pi\mu \cos r \cdot d/\lambda$$

where minima correspond to even numbers of π. Hence the positive field of the chart applies at once. The equation as written is primarily useful for the wedge. What the diagram points out very well is the resolution of doublets in the spectra of very high order. If the incidence is normal and the wedge be of air as in the interferometer $\phi = 2\pi d/\lambda$. If the thickness is constant and the angle of incidence varies the oblique lines are still applicable if instead of d the quantity $\cos r$ be laid off as the abscissa. They are real between $r = 0$ and the angle of total reflection.

In practice the oblique lines may be drawn to the scale on a sheet of paper, the black bars on a sheet of celluloid capable of sliding up and down over the former and the vertical lines may be represented by threads movable to right and left over the celluloid. The whole is to be serviceably framed on a sheet of tin plate.

C. Barus

Brown University,
Providence, R. I.

THE CAUSES OF THE GLACIAL EPOCH[1]

Of the various hypotheses advanced to account for the occurrence of a protracted glaciation of a large portion of the earth's surface in post-tertiary times, none has thus far met with universal acceptance; partly because of the insufficiency of data regarding critical geographical areas, but mainly because of the feeling that although the causes discussed by Croll, Chamberlin and others are or may be true ones, they are inadequate to account, quantitatively, for all the facts observed. The paleontological evidence of the prevalence of temperate and even semi-tropic floras and faunas in the late Tertiaries within what are now arctic regions, gives such forcible evidence of the comparative uniformity of tem-

[1] Read at tenth session of the International Geological Congress, Mexico, September 6–14, 1906.

perature prevailing even in late preglacial times all over the globe, that we instinctively seek for some more cogent cause of the widespread glacial phenomena that followed so closely upon the periods of greatest mountain-making recorded in geological history.

It is not the purpose of this paper to discuss the glacial theories alluded to above in detail, that having been done by many able writers; but rather to call pointed attention to another hypothesis now before the scientific world for a number of years, and which seems to me to offer a complete solution of the problem, provided it can pass the criticism of physicists; for from the geological point of view it seems to suffer no valid[2] objections. This hypothesis or theory (for it seems rather to deserve the latter name from its comprehensiveness) first brought forward by Dr. Marsden Manson in 1891, has until lately suffered the initial fate of many others now generally accepted, viz., that of being 'todtgeschwiegen' at first, as being too much opposed to some generally accepted, but by no means proved dicta regarding earth-heat, and especially the time-limit of its influence upon terrestrial surface-temperatures. Physicists at one time claimed that the globe as a whole is more rigid than steel, since it must otherwise suffer sensible tidal deformation; but were met by the ocular proof, known to every geologist, that so far from being even moderately rigid, the crust as known to us is a mere congeries of fragments in unstable equilibrium and in more or less constant movement for readjustment.[3] We are now told that the transmission of earth-heat to the surface must have ceased in early geological time, because of the low conductivity of the rocks known to us. But the enormous effusions of molten rock even in late Tertiary times, followed by manifestations of vulcanism which, though now apparently in course of extinction, proves the continued existence of high temperatures not far below the surface crust, again invalidates the physicists' objection, because based on the arbitrary assumption that conduction was the main or only manner in which the interior heat could reach the surface.

It is evident that whatever may have been the original source of that heat, whether from cosmic contraction or planetesimal collision, it has existed and exists now, even though practically insensible at the earth's surface. Even if, as some believe, to-day's vulcanism were only 'skin-deep,' the evidence of former much greater heat is too strong to be set aside. That this heat must have been brought up from the depths of the earth-mass by water, aqueous vapor and other gases, as is now the case in volcanic eruptions, and is also being done by

[2] While it is evident that extended glaciation existed in the Permian era, the claim that it was as extensive and continuous as the Pleistocene glaciation does not seem to me to be well established. The Permian having been, as is strongly emphasized in Chamberlin and Salisbury's 'Handbook of Geology,' a time of extended deformation of the earth's crust, to which the deflection of ocean currents, causing glacial conditions, is ascribed by them, it seems quite reasonable to suppose that such deformations extended to the upraising of large areas, together with orogenic uplifts, of which no evidence now remains except the seaward borders of the glaciated areas, with their till and roches moutonnèes, such as we now see at the foot of glaciers. It is pertinent to inquire, in this connection, by what imaginable changes in land areas and ocean currents the north and south polar regions could *at this time* remain deglaciated and made to grow magnolias, figs and the like. The Arctic Ocean is now substantially closed to equatorial currents; the Antarctic is wide open; yet both are glaciated. What intermediate arrangement could give either or both a temperate climate, with or without more, or less, carbon dioxid?

The claim that aridity is shown by the prevalence of evaporation-deposits of rock-salt and gypsum, is hardly tenable alongside of that made for glaciation. The low temperature and abundant moisture required for glaciation do not seem compatible with arid heat. Evaporation due to currents of undersaturated air, such as always characterizes descending air currents, is very effective; and the arid conditions would hardly be expected to extend as far north as Stassfurt.

[3] See, for the latest discussion of this subject, the paper on 'The Geodetic Evidence of Isostasy,' etc., read before the Washington Academy of Sciences by John F. Hayford, May 18, 1906.

geysers, can not be questioned. Whatever part chemical action, the friction or collisions connected with faults, or that accompanying flexures of strata may have in generating heat, that heat-generation must have been much more active in times of higher original temperatures and active mountain-making. It seems as though, despite all contrary suggestions thus far made, a higher temperature at the earth's surface than exists at present, within the geological ages covered by the warm-temperate flora and fauna found also in the arctic regions, may fairly be presumed on physico-geological grounds alone. This reasonable assumption forms the basis of Manson's theory of the ice age.

It can not be doubtful that during any highly heated condition of the globe, of whatever origin, the bulk of the water now gathered in seas, lakes and rivers existed in the form of vapor, which as it ascended was condensed into a mass of clouds forming a thick spheroidal envelope all around. On the outside, upper surface of this cloud-sphere the sun exerted substantially the same zonal effects as it now does upon the earth's surface, modified mainly by the uniformity of the physical nature of the cloud-surface, as against the alternation of sea and land as they now exist, and which by their differences in the absorption and radiation of heat, in heat capacity, and in topographic features, modify profoundly the typical, regular zonal order of climates. The tropical belt with its strong ascending currents, low barometer, and high temperatures; the two adjoining arid belts with descending currents and high barometer, and the temperate zones to poleward of the same, with variable but generally low barometer, would be defined on the cloud-spheroid as they are now on the earth's surface, but with greater regularity, though perhaps less sharply. It is also clear that, though not directly influencing the temperature of the earth's surface, the solar radiation would act powerfully as a conservator of earth-heat, compensating to some extent the radiation into space from the cloud-surface, of the heat carried up by convection currents.

The general disposition of the rain-belts would also be substantially as it is now, but the amounts of rainfall would, in so thick a cloud-cover, undoubtedly be greater than at present. The isothermal spheroids or shells corresponding to our present temperatures would at first be at heights more considerable than at present; but as the heat carried up from the earth's surface was more and more lost by radiation into space from the exterior cloud-surface, the isothermal shells would gradually descend, and the temperature of the falling rains would become lower, so as under favorable conditions to fall as snow. It is clear that snowfall might occur at any period of the earth's evolution on high mountain ranges or plateaus, and there the accumulation of snow might at any period have formed nevees and glaciers with their well-known effects. The earlier glaciations observed, especially in the Permian, are, therefore, quite compatible with Manson's theory. Elevation as a cause of glaciation must, however, be accompanied by its necessary correlative factor, an abundant rainfall; a point frequently left out of consideration in this connection. Labrador is a conspicuous example of non-glaciation from low precipitation.

Owing to the higher radiating power of the earth-surface as compared with the ocean, as well as to its much lower specific heat, the earth must have cooled more rapidly than the oceans by radiation alone. In addition to this, the water flowing from it into the seas would carry off a large amount of heat. Even while the ocean still received heat from its bed, the land areas would be a cooling agency especially for the ocean depths, while the warm oceanic surface waters would be supplying abundant vapor for precipitation on the relatively colder land areas. The latter would finally fall to so low a temperature as to receive their precipitation in the form of snow, thus inaugurating the glacial period, during which the isothermal shell of say the freezing-point of water, and below, descended near to the earth's surface. As the ocean gradually also cooled and evaporation diminished, the protecting cloud-envelope became thinner, first in the tropics and the flanking belts of lesser

rainfall (which later became the arid belts); and thus gradually the zonal solar régime was established.

Such are main features of Manson's theory, the details of which have been elaborated in his published treatise on the evolution of climates and other papers, and the substance of which will be presented to this body by the author himself. In my view it is not a conception to be lightly set aside, for whatever evidences of former glaciations may have been observed, there has not appeared in former geologic history anything resembling in magnitude the pleistocene glaciation, the scattered remnants of which are even now in gradual retreat under our eyes. The observed evidences of glaciation in former geologic ages do not appear to be of such extent, or to be accompanied by detrital deposits indicating a continental extent of glaciation; they are apparently such as might be produced at any time by either the upthrusting of mountain chains, or by wider, epeirogenic elevations of the surface. Thus far, it seems as though there had been but one distinctively glacial epoch of world-wide importance and extent; and that nearly contemporaneous with the appearance of man upon earth.

It has been asked how the early floras and faunas could have existed and developed under the perpetual cloud assumed by Manson's theory to have covered the earth prior to the establishment of the solar climate, toward the end of the glacial period. In answer to this it may be suggested, apart from the fact that even at the present time the average cloudiness of the sky is estimated at 60 per cent., that the earlier floras consisted almost exclusively of plants whose analogues or evolutional successors, such as ferns and horse-tail rushes, vegetate preferably in dense shade, even in cloudy climates; and the extreme succulence of the carboniferous flora is sometimes approached when in certain climates, under unusally rainy seasons, such plants grow to maturity almost without a ray of sunshine. The plants growing under the canopy of primæval forests, in perpetual twilight, show how easily vegetation adapts itself to such conditions. In later periods, as the cloud-envelope brightened, the higher orders of plants, now preferably basking in sunshine, had opportunity to develop to their present prominence. But it is notable that the present forms of peculiarly arid-region plants, which are specially adapted to hot sunshine and dry air, are absent from any of the fossil forms thus far reported. They clearly had no *raison d'être* until the cloud-veil was dissipated by the sun.[4]

There seems to be as little difficulty in assuming the animal creation to have been tolerant of, or adapted to, a sunless existence. Not to speak of our present nocturnal and deep-sea faunas, the adaptability of the pupil of the eye now existing provides all needful conditions so far as vision is concerned; and the great wide-open orbs of the ichthyosaurs suggest ready adaptation to dark days. Here

[4] Chamberlin (SCIENCE, October 26, 1906) claims that the existence of palisade cells in plants of paleozoic age proves the existence of arid-region plants at that time. But palisade cells as such depend much less upon climatic factors than upon leaf-texture and botanical relationship. It is only the presence of *several* tiers of such cells beneath the epidermis of the upper leaf-surface that constitutes such presumptive proof; witness the existence of abundant palisade cells in firm-leaved ferns that are at home in the deepest shade, right alongside of others which show no such tissue; as well as the abundant palisade tissue in the leaves of the shade-loving Pyrolaceæ and other ericaceous plants, of *Vinca minor*, *Myosotis palustris* and thousands of other shade plants. Moreover, *saline* soils cause xerophytic structure and growth; which, therefore, should not surprise us if found in coal plants. The very generally clayey (fire-clay) nature of the substrata of coal beds plainly suggest that the coal-forming flora was one of *swamp* plants, and not xerophytic or even upland, as suggested by Chamberlin. So far as I am aware, no plants showing the well-known extreme provisions against drying-out, such as we find in the cactus and others, have been found among the fossils of even the late Tertiary. On the other hand, the fauna of the Permian, belonging chiefly to the Brachiopod and Cephalopod orders, indicates a warm temperate or tropical, not a frigid temperature of the seas, such as is shown by the marine fauna of the Pleistocene glacial epoch.

as in the case of plants, the organisms specially adapted to continual sunshine—the desert fauna—seem to be absent from prepleistocene deposits. Horned toads, Gila monsters and animals of similar habits were not then in evidence, so far as the writer is aware.

In any case, the postulates for a sunless existence of prepleistocene beings are not greater, if as great, as those involved in Chamberlin's hypothesis of a materially greater, or less, content of carbonic dioxid in the atmosphere.

It does seem to the writer that unless it can be shown that the temperature prevailing at the beginning of the glacial epoch could not have been high enough to maintain a cloud envelope, Manson's theory as outlined above must be considered the most probable among those that have heretofore been suggested, as fulfilling both qualitatively and quantitatively the postulates of the great ice age; not excluding, of course, the probable influence of the agencies claimed by Arrhenius and Chamberlin as the chief ones, but which appear to the writer to be inadequate to account for the phenomena in actual evidence.

E. W. Hilgard

University of California,
January, 1907

CURRENT NOTES ON METEOROLOGY

WINDS ON THE PEAK OF TENERIFFE

The controversy regarding the direction of the upper winds in the vicinity of the Cape Verde and Canary Islands has prompted Hann to bring together (*Met. Zeitschr.*, Dec., 1906) the published observations of wind direction on the summit of the Peak of Teneriffe (Lat. 28° 49′ N.; Long. 16° W., altitude 12,172 feet). The conclusion is as follows: the S. W. and W. wind which is very often observed above 3,000 meters, even in summer, is certainly not a local wind, but belongs to the upper members of the general atmospheric circulation. The N. E. trade occasionally blows on the top of the peak, at least in summer. The mean direction of cirrus clouds in winter is W. by S. The N. W. winds observed by Hergesell in summer in the vicinity of the Canary Islands were probably connected with the then location of the subtropical high-pressure area of the North Atlantic Ocean. The map of isobars at 4,000 meters (Teisserenc de Bort) shows, in July, the center of the maximum somewhat N. W. of the Canaries, so that northerly winds at 4 km. above sea-level would not be contradictory to the pressure distribution. According to the interesting observations of temperature and humidity made by Hergesell in the free air above the anticyclone, the latter may extend to greater altitudes than has thus far been assumed. More constant equatorial currents are to be expected over the West Indies and Central America in the same latitudes.

CLIMATOLOGY OF THE UNITED STATES

'The Climatology of the United States,' by Professor A. J. Henry (Bull. Q, U. S. Weather Bureau, 4to, 1906, pp. 1012, Pls. 34, Figs. 7), is one of the most important publications of our Weather Bureau. The need of a compact summary of the essential climatological data for the United States has long been felt. Hitherto these tabulations have been scattered through various annual reports of the chief of the Weather Bureau. Since Loren Blodget's famous classic, 'The Climatology of the United States' (1857) there has been no attempt to collect into one volume, and to discuss, the mass of climatological material collected by our official and voluntary observers. The data in this volume cover, generally speaking, the period 1870–1893. There is a discussion, satisfactory on the whole, of the climates of the United States in general (84 pages), illustrated by a considerable number of maps. This is the portion of the book which will be most generally used, and it will serve its purpose well. A long series of tables follows, in such form that they can easily be referred to by those who wish detailed information. At the end, occupying the larger part of the volume, come condensed summaries for the different states. The advertised price of the book is $10, which is much too high if the volume is to find its way

generally into our libraries, but it is to be hoped that all educational institutions will secure free copies.

THE ANTI-TRADE OVER THE ATLANTIC OCEAN

THE 'Results of the Franco-American Expedition to explore the Atmosphere in the Tropics' are discussed by Professor A. L. Rotch in the *Proc. Amer. Acad. Arts and Sci.*, Vol. 42, No. 14, Dec., 1906. A summary of these results has already appeared in SCIENCE. This expedition, it will be remembered, was sent out in the summer of 1905, at the joint expense of Messrs. Teisserenc de Bort and Rotch, and made studies of the atmospheric conditions in and above the N. E. trade belt of the eastern North Atlantic, by means of small balloons and kites. The most important result of the summer's work was the establishment of the fact that 'the classic observations of the return trade, which were long ago made on the Peak of Teneriffe, indicate a general phenomenon, and agree with those obtained over the open ocean by the present expedition.' We note also a confirmation (p. 268) of the view that at sea cumulus clouds (noted at the edge of the N. E. trade in this case) are probably formed by the condensation of water vapor which is diffused upward from the ocean surface.

THE TSUKUBA OBSERVATORY

WE have received the results of meteorological observations made on Mt. Tsukuba (Japan) during the year 1902. This mountain is about forty miles northeast of Tokio, and rises to 2,854 feet at its highest summit. Although the altitude is not great, the exposure is excellent. In addition to the summit station, there are also a base and an intermediate station. The observatory is well equipped with standard instruments. Observations are taken at 2, 6 and 10 A.M. and P.M. on the summit; at 0, 2, 4, 6, 8, 10 A.M. and P.M. at the intermediate station. At the base, observations are made weekly, when the thermograph and barograph sheets are changed. On the summit, hourly records can be obtained from the self-recording instruments. An annual publication is issued. On the title-page appears this statement: 'Herausgegeben von Hofmarschall-Amt. S. K. H. des Prinzen Yamashina.'

TUBERCULOSIS AMONG THE INDIANS OF ARIZONA AND NEW MEXICO

UNDER the above title Dr. I. W. Brewer, of Fort Huachuca, Ariz., has given the results of a study recently made by him, with the assistance of the medical officers at the Indian agencies and schools (*N. Y. Med. Journ.*, Nov. 17, 1906). The wide-spread prevalence of tuberculosis among these Indians emphasizes very forcibly the fact that the climate of Arizona and New Mexico, with all its sunshine and dryness, is not a specific. No climate *is* a specific. It is certainly of great benefit to those in the early stages of tuberculosis, but is of little value when a patient is improperly nourished and is surrounded by filth, or lives in poorly ventilated houses.

R. DEC. WARD

HARVARD UNIVERSITY

THE AMERICAN WOMEN'S TABLE AT NAPLES

THE Naples Table Association for promoting Laboratory Research by Women wishes to call attention to the opportunities for research in zoology, botany and physiology provided by the foundation of this table.

The Zoological Station at Naples was opened by Professor Anton Dohrn in 1872 for the collection of biological material and for the study of all forms of plant and animal life. Under the personal direction of Professor Dohrn and his assistants the station has developed into an international institution for scientific research. Any government or association which pays five hundred dollars annually is assigned a table for research and is entitled to appoint to it qualified students, who are provided by the station with all materials, apparatus and assistance, free of cost. One table is sometimes used by four or five research students in the course of a year.

This association, which was formed in 1898 to promote scientific research among women,

is maintained by annual subscriptions of fifty dollars each. For the year 1906–7 the following colleges, associations and individuals are contributors: Association of Collegiate Alumnæ, Barnard College, Bryn Mawr College, University of Chicago, Mass. Institute of Technology, Mount Holyoke College, Radcliffe College, Smith College, University of Pennsylvania, Vassar College, Wellesley College, Western Reserve University, Women's College in Brown University, Women's Advisory Committee of the Johns Hopkins Medical School, Woman's College of Baltimore, Miss Helen Collamore, Mrs. Alice Upton Pearmain, Mrs. J. M. Arms Sheldon, Mrs. Elizabeth A. Shepard, Mrs. Mary Thaw Thompson.

The year of the association begins in April, and all applications for the year 1907–8 should be sent to the secretary on or before March 1st, 1907. The appointments are made by the executive committee.

A prize of $1,000 has been offered periodically by the association for the best thesis written by a woman, on a scientific subject, embodying new observations and new conclusions based on an independent laboratory research in biological, chemical or physical science. The fourth prize will be awarded in April, 1909.

Application blanks, information in regard to the advantages at Naples for research and collection of material and circulars giving the conditions of the award of the prize will be furnished by the secretary.

Executive Committee: Florence M. Cushing, 8 Walnut Street, Boston, Mass., chairman; Mary E. Woolley, President of Mount Holyoke College; Ellen H. Richards, Massachusetts Institute of Technology; Alice Upton Pearmain, 388 Beacon Street, Boston, Mass.; Marion Talbot, Dean of Women, Chicago University; Elizabeth L. Clarke (Mrs. S. F.), Williamstown, Mass., treasurer; Ada Wing Mead (Mrs. A. D.), 283 Wayland Ave., Providence, R. I., secretary.

THE ROCKEFELLER INSTITUTE FOR MEDICAL RESEARCH

The Rockefeller Institute for Medical Research purposes to award for the year 1907–8 a limited number of scholarships and fellowships for work to be carried on in the laboratories of the institute in New York City, under the following conditions:

The scholarships and fellowships will be granted to assist investigations in experimental pathology, bacteriology, medical zoology, physiology and pharmacology and physiological and pathological chemistry.

They are open to men and women who are properly qualified to undertake research work in any of the above-mentioned subjects and are granted for one year.

The value of these scholarships and fellowships ranges from eight hundred to twelve hundred dollars each.

It is expected that holders of the scholarships and fellowships will devote their entire time to research.

Applications accompanied by proper credentials should be in the hands of the secretary of the Rockefeller Institute not later than April 1, 1907. The announcement of the appointments is made about May 15. The term of service begins preferably on October 1, but, by special arrangement, may be begun at another time.

L. Emmett Holt,
Secretary

14 West 55th Street,
New York City

SCIENTIFIC NOTES AND NEWS

The Rumford medal of the American Academy of Arts and Sciences 'for discoveries in light and heat,' has been awarded to Professor E. F. Nichols, of Columbia University.

The Berlin Academy of Sciences has conferred its Helmholtz medal on M. Henri Becquerel, Paris.

Dr. Eduard Zeller, the eminent student of the history of philosophy, has celebrated his ninety-third birthday.

Dr. H. H. Hildebrandsson, professor of meteorology and director of the Meteorological Institute of the University of Upsala, and Professor Knut Joh. Ångström, professor of physics, have been elected honorary members of the Royal Institution, London.

DR. T. W. RICHARDS, professor of chemistry at Harvard University, has received a fifth grant of $2,500 from the Carnegie Institution of Washington.

MR. A. F. BURGESS, secretary of the Association of Economic Entomologists, has tendered his resignation as state inspector of orchards and nurseries for Ohio, to go to Massachusetts to experiment with insecticides for the destruction of gypsy and brown-tail moths.

PROFESSOR HENRY F. OSBORN is expected to return about April 1 from Egypt, where, with the assistance of Messrs. Walter Granger and George Olsen, he has been making paleontological explorations in the Fayoum desert on behalf of the American Museum of Natural History.

PROFESSOR HAROLD HEATH, of Stanford University, has been given leave of absence for the second half year, and will go first to Naples. He will return by way of Japan and the Malay Archipelago.

WE learn from the New York *Evening Post* that an archeological expedition, under the direction of Professor John R. S. Sterrett, will start on March 9 for Asia Minor. Professor Sterrett has previously spent several years in exploration and excavation in the east. He will be accompanied by B. B. Charles, instructor in Semitics, A. T. Olmstead, formerly fellow in the American School at Jerusalem and now at Athens, C. O. Harris, late instructor in Latin, now in the American School at Athens, and J. E. Wrench, late fellow at the University of Wisconsin.

THE Society for Horticultural Science, of which Professor L. H. Bailey, of Cornell University, is president, and the American Pomological Society, of which L. A. Goodman, Kansas City, is president, will meet on the grounds of the Jamestown Exposition on September 25 and 26.

PROFESSOR ERNEST RUTHERFORD gave a lecture at Clark University on February 15 on 'Radium and Radioactive Substances.'

PROFESSOR W. T. COUNCILMAN, of the Harvard Medical School, delivered an address on January 23, before the Harvard Club of Worcester, on 'The New Medical School in its Relation to the University.' He also gave the opening address at the Tuberculosis Exhibit at Taunton on January 31. His subject was 'Tuberculosis: the nature of the disease and the modes of relief.'

THE annual address before the Society of the Sigma Xi was given at the University of Nebraska on February 15 by Dr. Charles Sedgwick Minot, of Harvard University, on 'The Biological Interpretation of Life.'

MR. W. BATESON gave, on February 11 and 15, two lectures on 'Mendelian Heredity and its Application to Man,' in the medical schools of Cambridge University.

THE Boston *Transcript* states that the fund which a special committee has been instrumental in assembling to be used for a memorial to the late Professor Shaler, of Harvard University, has been completed, and amounts to $30,000. The committee, of which Edward W. Atkinson is chairman, will meet to decide upon the exact form the memorial will take.

THE Paris Municipal Council has appropriated the sum of about $800 for a monument in honor of Pierre Curie, to be erected in the School of Physics and Industrial Chemistry.

THE Botanical Seminar of the University of Nebraska is to celebrate the two hundredth anniversary of the birth of Carl von Linne on the eleventh of May next, the day before the exact anniversary. Addresses are to be made by Dr. Clements, Dr. Pound and Dr. Bessey, in connection with the commemorative exercises.

DR. J. PÖSCHL, professor of physics at the Technological Institute at Graz, has died at the age of seventy-nine years.

THE deaths are also announced of Professor le Roux, formerly professor of physics at the Paris School of Pharmacy, and of Dr. Lyon, docent for analytical chemistry at Geneva.

THERE will be a civil service examination on March 20 and 21 for the position of scien-

tific assistant in veterinary zoology in the Bureau of Animal Industry at a salary of $840.

The zoological and ethnical collections made recently in East Africa by Mr. Richard Tjäder have been acquired by the American Museum of Natural History.

We learn from English journals that the pearl oyster fisheries of the Mergui Archipelago, lying off the province of Tenasserim, Lower Burmah, are to be the object of an investigation on behalf of the Indian government, and for this purpose Mr. R. N. Rudmose Brown and Mr. J. J. Simpson left early last month for Rangoon. It is extremely probable that an examination of the ground may result in the discovery of new pearl banks, or at least the possibility of such banks being started. It is expected that the investigation, at least on its economic side, will be completed before the commencement of the southwest monsoon season in May.

The lichen collection of Dr. H. E. Hasse, of California, consisting of about 3,000 species and many duplicates, has been recently presented to the New York Botanical Garden by Mr. John I. Kane.

The New York Aquarium, situated in Battery Park, and conducted by the New York Zoological Society, with Dr. C. H. Townsend as director, is open free, every day in the year. It is closed on Monday forenoons except to school teachers with their classes, and to members of the New York Zoological Society. When a holiday occurs on Monday the public is admitted as on other days. The attendance for the ten years ending December 31, 1906, amounted to 17,103,328—an average of 4,685 visitors a day. The attendance for the year 1906 was 2,106,569—an average of 5,771 a day.

Five free lectures on popular scientific subjects, illustrated with lantern views, and open to the public, are being given in the Geological Lecture Room, University Museum, Harvard University, at 3:30 o'clock on Sunday afternoons, February 3 and 17, March 3, 17 and 31, 1907. The subjects treated in the lectures are related to exhibits in different parts of the Museum.

February 3—'Meteorites, their Fall from the Sky, their Composition and their Relation to the Rocks of the Earth's Interior,' Professor J. E. Wolff.

February 17—'The Ruins and the Ancient People of Yucatan, Mexico,' Dr. A. M. Tozzer.

March 3—'Why the Earth is believed to be Millions of Years Old,' Professor W. M. Davis.

March 17—'Tropical Plants,' illustrated by Museum Specimens and Pictures, Professor G. L. Goodale.

March 31—'Quartz, its Varieties, Origin, Characteristics and Uses,' Professor Charles Palache.

We learn from the *American Museum Journal* that the American Bison Society held its annual meeting at the Museum on Thursday, January 10. This society has for its object not only the prevention of the extermination of the bison, but also the encouragement of the raising of the animal as a commercial proposition. A generation ago the bison, or American buffalo, roamed over the western plains in vast herds, estimated to contain more than ten million individuals, while to-day, on account of the merciless and wanton slaughter practised in the early eighties, scarcely two thousand are known to be in existence. The society proposes to encourage the establishment of bison reservations in each state where climate and other conditions are favorable for the maintenance and increase of herds. For New York the proposition is that, as a beginning, the state set aside nine square miles in one of the reserved areas of the Adirondack region and appropriate $15,000 for the purchase and maintenance of a herd of fifteen bison. Dr. William T. Hornaday, director of the New York Zoological Park, is the president of the society.

We learn from the London *Times* that Mr. Francis Galton, F. R. S., has given a further sum of £1,000, which has enabled London University to revise and extend the scheme for the study of national eugenics founded under his previous benefaction, and will provide for the carrying on of the work of the eugenics laboratory for the next three years. Mr.

David Heron, M.A., has been appointed Galton research fellow in national eugenics, in succession to Mr. Edgar Schuster, M.A., resigned; Miss E. M. Elderton has been appointed Galton research scholar, and Miss Amy Barrington (mathematical tripos, Cambridge) computer. The work in this subject will be carried on under the supervision of Professor Karl Pearson, F.R.S., in consultation with Mr. Francis Galton. It is the intention of the founder that the laboratory shall act (1) as a storehouse for statistical material bearing on the mental and physical conditions in man and the relation of these conditions to inheritance and environment, (2) as a center for the publication or other form of distribution of information concerning national eugenics. Provision is made in association with the biometric laboratory at University College for training in statistical method and for assisting research workers in special eugenic problems. Short courses of instruction will be provided for those engaged in social, anthropometric or medical work and desirous of applying modern methods of analysis to the reduction of their observations. The laboratory, which is in connection with University College, is temporarily established at 88, Gower-street, W.C.

WE learn from the London *Times* that the highways committee of the London County Council has presented a report to the council with reference to the Greenwich Electricity Generating Station and the Royal Observatory. Last year the admiralty appointed a special committee to inquire into the working of the station. The special committee, which consisted of Lord Rosse, representing the Royal Observatory, Professor J. A. Ewing, representing the admiralty, and Sir Benjamin Baker, representing the council, have now issued their report, and the conclusions arrived at are contained in the following recommendations, in which are suggested certain modifications in the arrangements at the generating station, and as to the manner and times at which the first portion of this station shall be worked, so as to avoid any possible interference with the work of the observatory: (*a*) The question, both as regards effects of vibration and obstruction through chimneys or discharge from chimneys, to be further reviewed after, say, two years, by which time experience should be obtained with the second portion of the station at work. (*b*) The generating plant for the second portion to be turbines, which, as well as the dynamos, must be of a perfectly balanced type, such as has been proved by trial not to cause vibration. (*c*) An undertaking to be obtained that when the plant in the second portion is available for use, the reciprocating engines of the first portion shall not in ordinary circumstances be used after 10 P.M., and their use shall be restricted as far as possible after 8:30 P.M. (*d*) The two chimneys of the second portion, at present incomplete, to be not higher than 204 feet above Ordnance datum. (*e*) The discharge of gases both from these and from the existing chimneys not to be materially hotter than the discharge is now from the existing chimneys—namely, about 250 degrees F. (*f*) No further extension of the station to be made beyond the 20,000 kilowatts now contemplated in the equipment of the second portion.

UNIVERSITY AND EDUCATIONAL NEWS

BY the will of the late John A. Creighton, the sum of $900,000 is bequeathed to educational and charitable institutions, including $500,000 to Creighton University. The residue of the estate which is said to amount to more than $5,000,000 is to be distributed *pro rata* to the same institutions, whence it appears that Creighton University will receive in all the sum of about $2,500,000.

TEACHERS COLLEGE, Columbia University, has received from a source not stated, a gift of $400,000 for a building for its school of domestic economy.

THE Johns Hopkins University has received $150,000 from the estate of the late Charles L. Marburg, $100,000, of which goes to the hospital and $50,000 to the university.

AN unnamed donor has presented a new gymnasium to Syracuse University.

TEN research fellowships of the annual value of $500 each have been established in

the engineering experiment station connected with the College of Engineering of the University of Illinois.

The University of Washington, Seattle, announces the establishment of five teaching fellowships in mathematics, each yielding annual stipends from $400 to $500. Fellowships are open to graduate students only.

The Hon. A. McRobert, Cawnpore, India, has founded a fellowship at Aberdeen University for cancer research. At Mr. McRobert's death the sum of $50,000 is to be available for this work, but that it may begin at once he has undertaken to provide $2,000 a year.

The will of the late Charles James Oldham, of Brighton, leaves to the University of Oxford and to the University of Cambridge the sum of £5,000 each, such sums to be invested. The incomes arising from such investments are to be applied to the founding of one annual prize or scholarship in the ancient classics, Greek and Latin, and one annual prize or scholarship in the knowledge of William Shakespeare's works, such prizes or scholarships to be called the 'Charles Oldham' prize or scholarship.

President Needham, of George Washington University, has announced that Van Ness Park, purchased two years ago as the new site of the university, had been sold to the United States government as the site for the new building of the International Bureau of American Republics; and that the university held an option on 'Oak Lawn,' a tract of land at the head of Connecticut Avenue, which could be bought for $800,000. He stated that $400,000 was already in hand, and that Theodore J. Mayer of Washington had offered to erect a building to cost $185,000, on condition that this site be selected.

Oberlin College will celebrate the seventy-fifth anniversary of its foundation from June 19 to 25.

Dr. Henry Pratt Judson, professor of political science in the University of Chicago, and since the death of Dr. Harper acting-president, was elected president of the university on February 20.

It is understood that the presidency of the University of Toronto has been offered to Dr. M. E. Sadler, professor of education at the University of Manchester, formerly director of special enquiries and reports in the British Education Department.

Mr. George H. Locke, of Ginn and Company, recently dean of the College of Education of the University of Chicago and editor of the *School Review*, has been appointed dean of the School for the Training of Teachers in the Macdonald College, founded by Sir William Macdonald and affiliated with McGill University, Montreal. The new building of the college, of which Dr. James W. Robertson is the director, will be opened in the autumn.

William H. Jackson, M.A. (Cambridge), lecturer at Manchester University, has been appointed professor of mathematics at Haverford College.

At Yale University, Dr. Charles H. Judd has been promoted to be professor of psychology, and Dr. F. P. Underhill to an assistant professorship of physiological chemistry. Dr. Ellsworth Huntington has been appointed instructor in geography and Dr. William E. Hocking, assistant professor of philosophy.

Dr. H. T. Barnes, associate professor of physics at McGill University, has been promoted to the chair of physics, vacant by the removal of Professor Ernest Rutherford to Manchester.

Dr. Carl M. Wiegand, of Cornell University, has been appointed associate professor of botany at Wellesley College.

Professor Georg Klebs, professor of botany at Halle, has been called to the chair at Heidelberg, vacant by the death of Professor E. Pfitzer.

Dr. E. D. Holzapfel, professor of zoology in the Technical Institute at Aachen, has been called to the University of Strasburg.

Dr. Ernst Meumann, of Königsberg, has been called to the chair of philosophy at Münster as successor to Professor Busse.